THE INSTITUTION OF PRODUCTION ENGINEERS JOURNAL

1957

THE INSTITUTION OF

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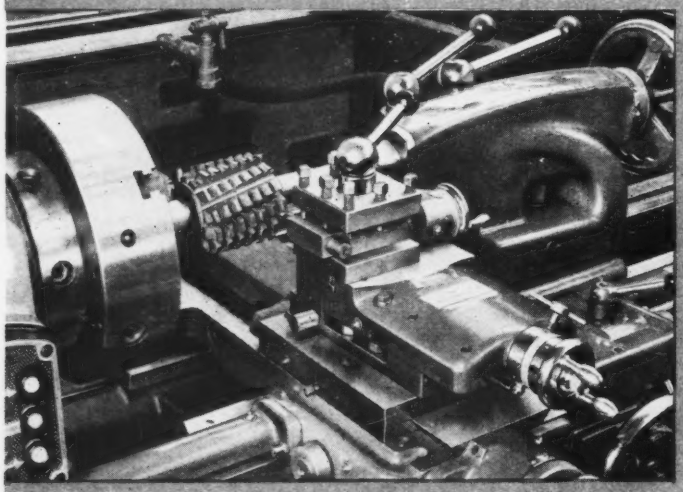

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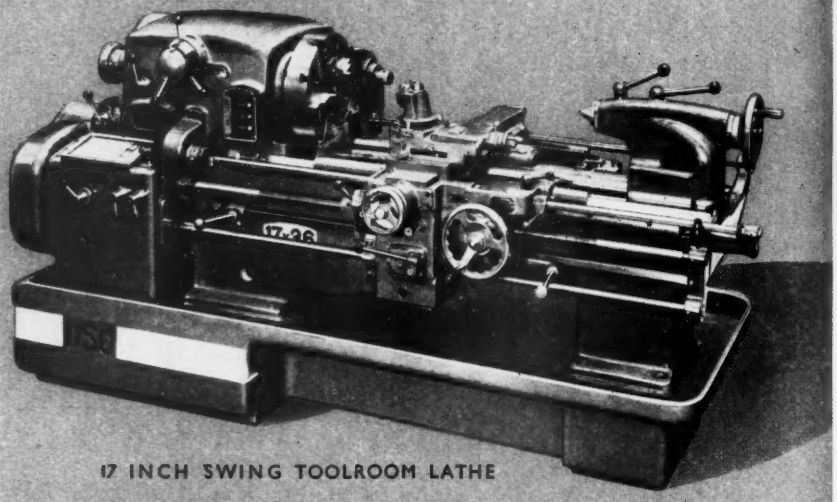

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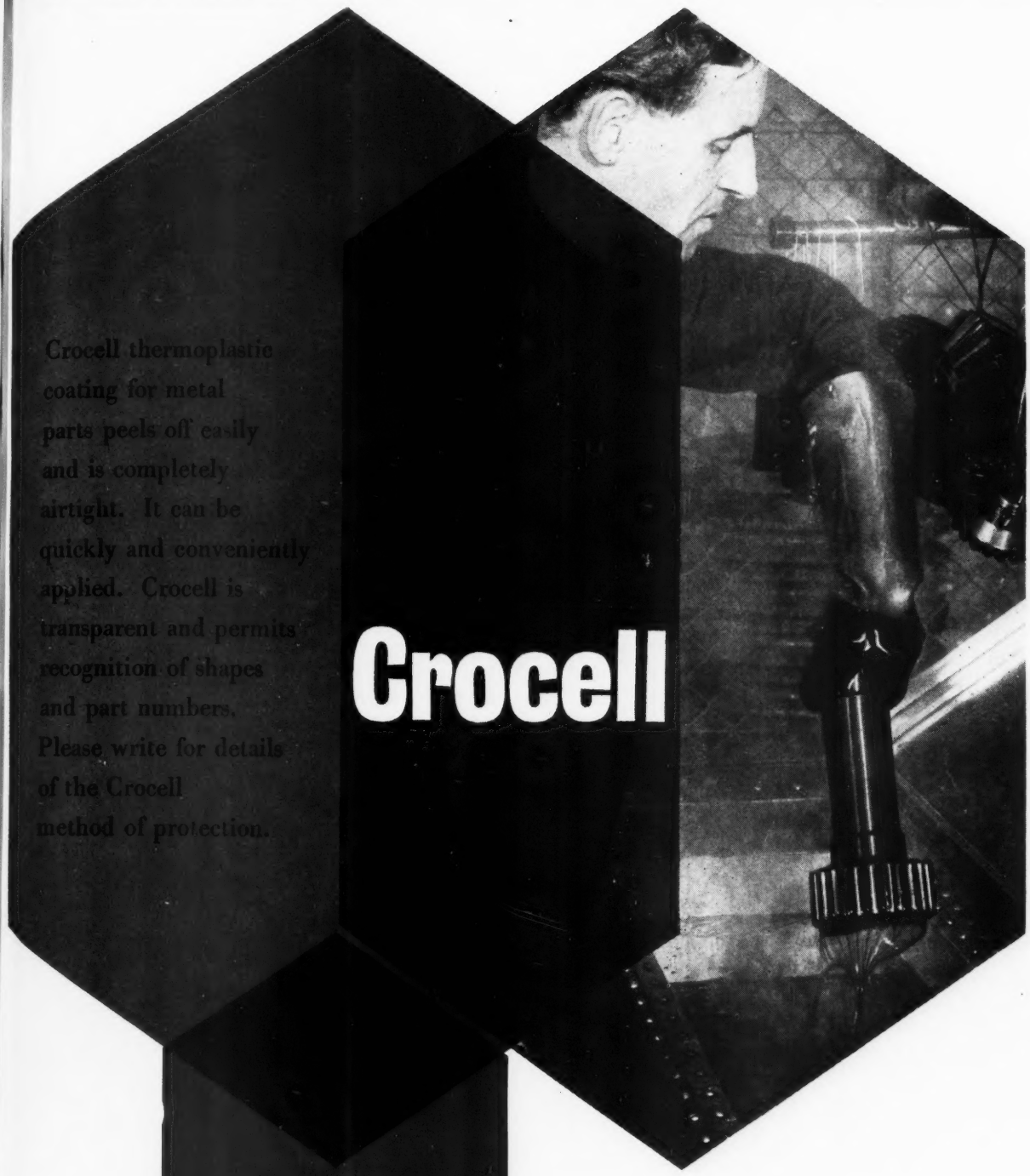

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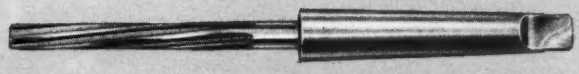

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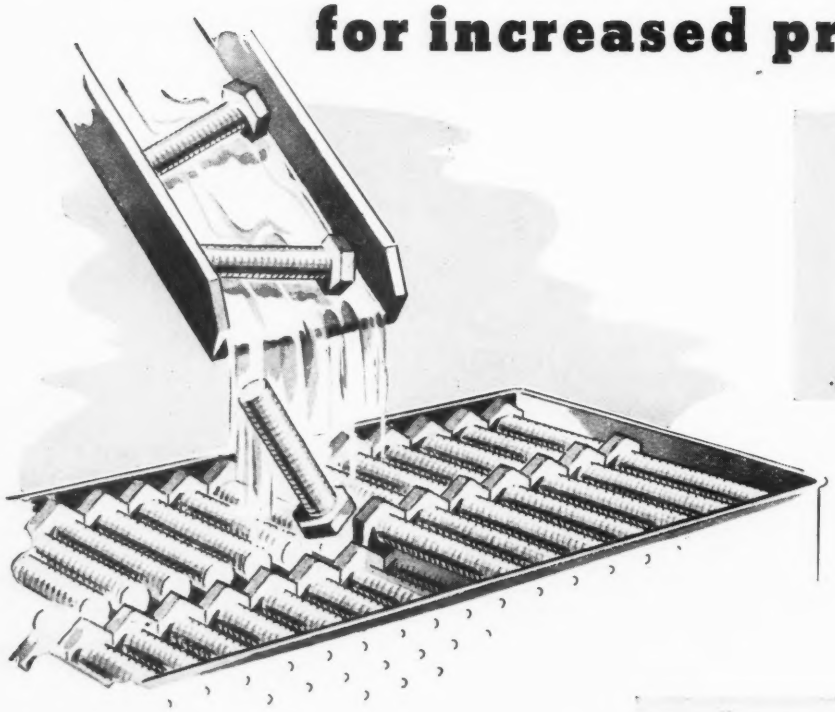

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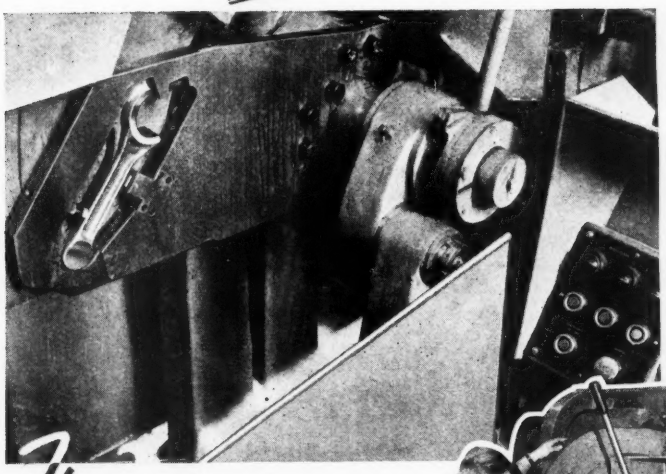

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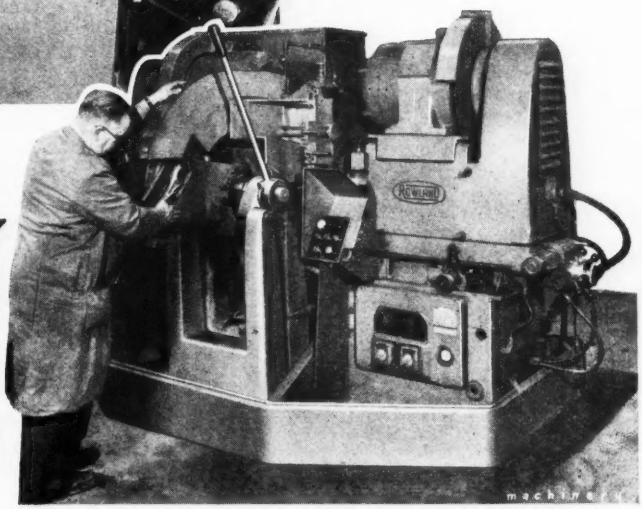

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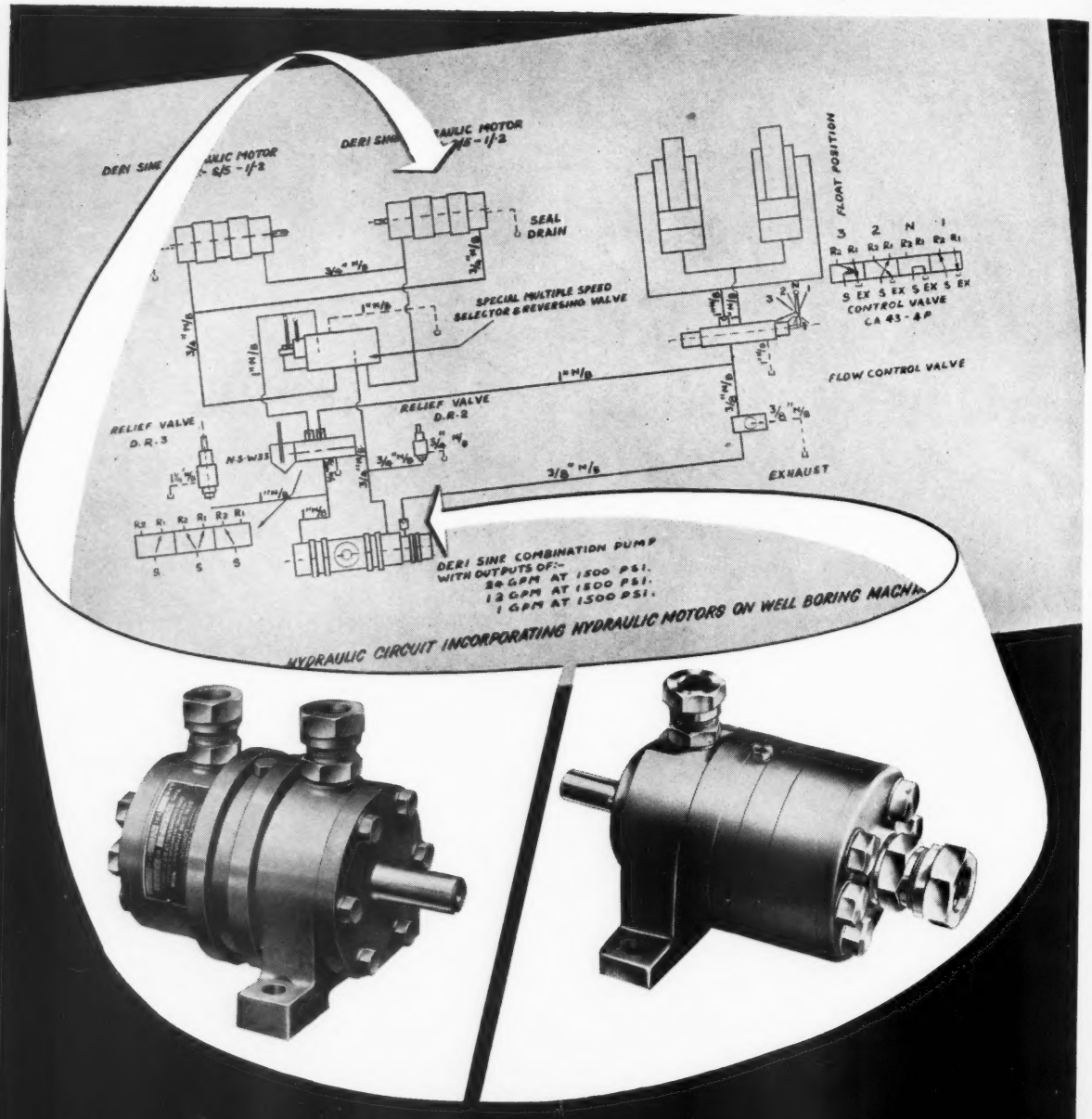
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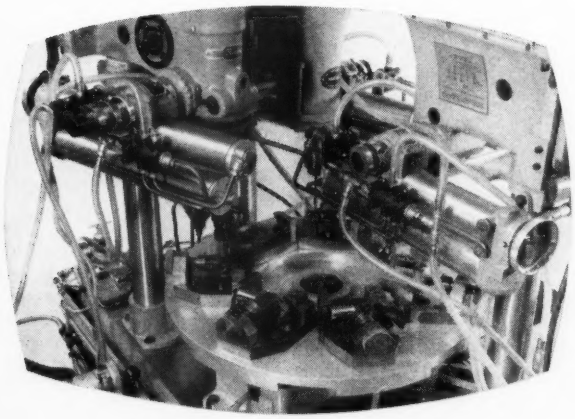

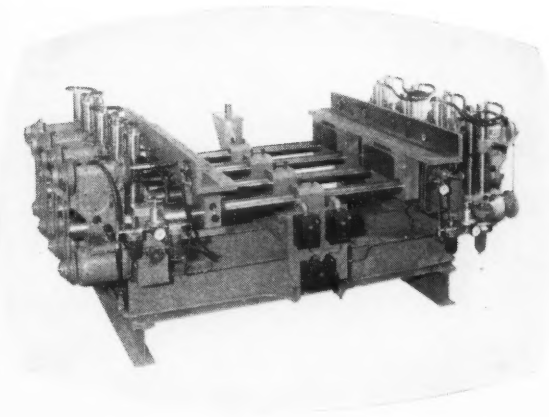

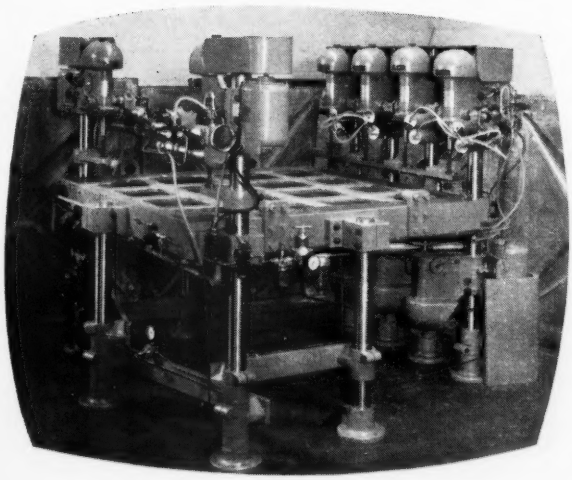

Above right: A machine for drilling 16 holes in a 54 in. x 52 in. component. 8 heads at 12 in. centres operate simultaneously. Component is automatically moved to next station and the operation repeated, leaving 16 holes at 6 in. centres. Floor to floor time 47 seconds; previously, the operation required 15 mins. 10 secs.

Above left: Progressive drilling followed by tapping on a special Hammer Head. Three operations performed simultaneously and in sequence with automatic table indexing. Floor to floor time 33 seconds; previously, the operation required 2 mins. 15 secs.

Below left: A machine for drilling 18 holes in a 54 in. x 52 in. component. Firstly 8 heads directly opposed at 6 in. centres operate simultaneously. The component is rotated through 90 deg. when 10 heads again operate to complete. Floor to floor time 47 seconds; previously, the operation required 15 mins. 10 secs.

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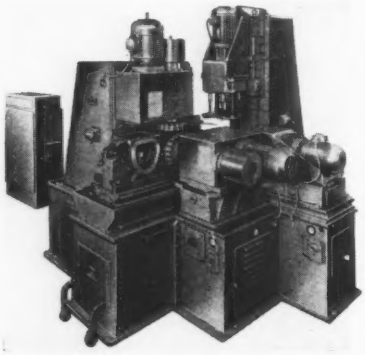
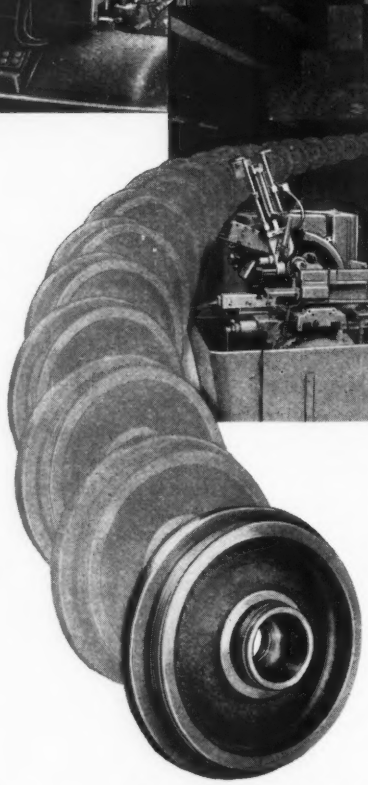
This is one of the possibilities of the automatic linking of B.S.A. machines. When the requirement is for medium or high-production, on standard or special purpose machines, consult B.S.A. Tools Ltd.



B.S.A. Hüller combined mill and drilling machine for transmission boxes.



THIS 4" dia. COMPONENT MACHINED COMPLETE on a B.S.A. 5M single-spindle automatic chucking machine (for 1st opn. work) linked with a B.S.A. ACME-GRIDLEY 6" six-spindle chucking machine. After 1st opn. machining the component is conveyed via the overhead chute to the multi-spindle machine. The component is transferred to the chucks electro-pneumatically within the automatic cycle of the machines.

B.S.A. TOOLS LIMITED · BIRMINGHAM 33 · ENGLAND

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From the B.S.A. range of single- and multi-spindle automatic bar and chucking machines, thread and form generators, multi-tool and copy turning lathes, centreless grinders, broaching machines, special purpose machines and small tools.

On his metal with Magnesium & M.E.L

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An outstanding service to top executives is provided by Magnesium Elektron Limited. As pioneers of British Magnesium for a quarter of a century, M.E.L's knowledge and unrivalled experience of the metal is invaluable to the executive.

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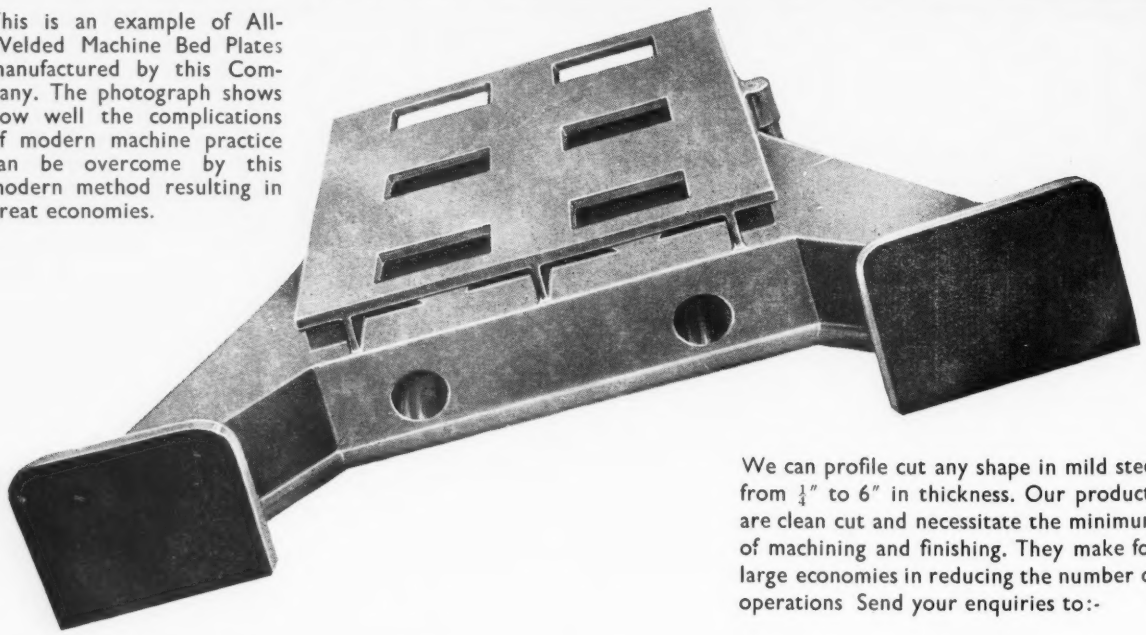
This is an example of All-Welded Machine Bed Plates manufactured by this Company. The photograph shows how well the complications of modern machine practice can be overcome by this modern method resulting in great economies.



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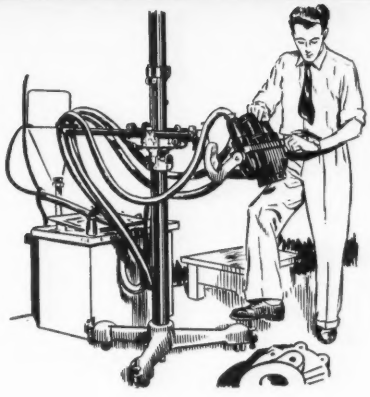

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The illustration above shows a 17-station in-line transfer machine for the complete machining of cylinder heads, both 4 and 6 cylinder types being handled on the same machine.
This is a typical example of the application of Asquith Unit Heads to the design of high output machine tools. Units from ½ H.P. upwards are utilized individually or in combination and mounted for horizontal, vertical or angular operation; the unit of appropriate H.P. is selected for each operation.
If your manufactures involve the large-scale production of specific components it is worth investigating the possibility of machining them on an Asquith Unit Head machine.

Write to Drummond-Asquith (Sales) Ltd. giving full details of the component and output required.

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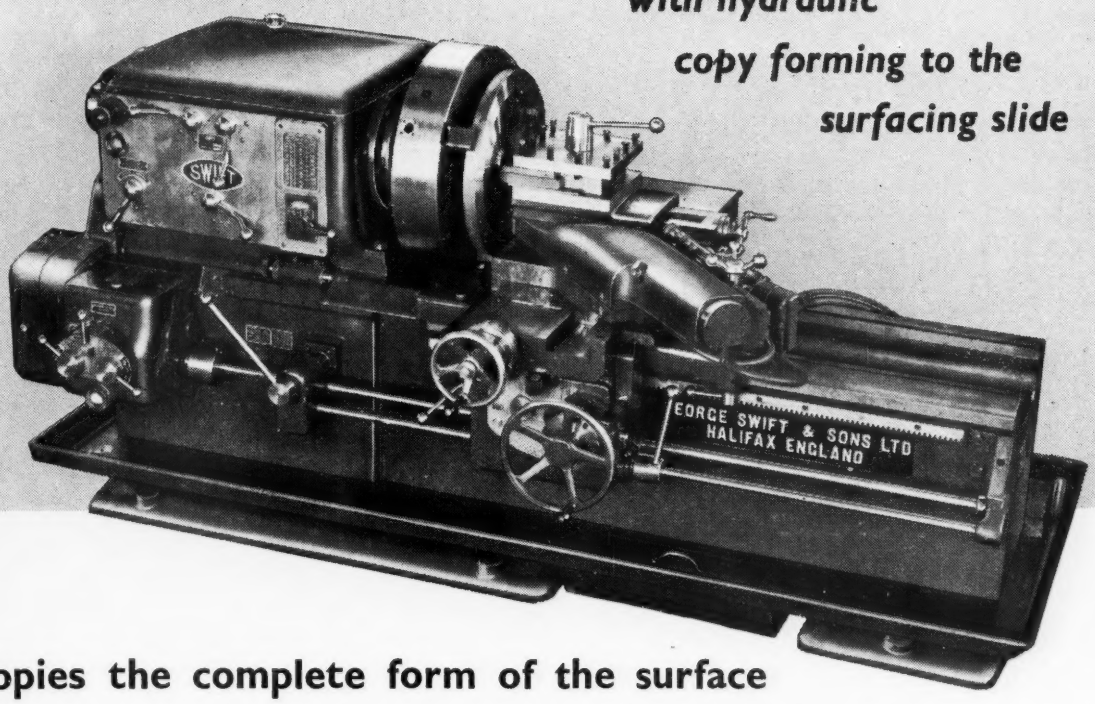
A 189

SWIFT

SURFACING AND BORING LATHE

with hydraulic copy forming to the surfacing slide



Copies the complete form of the surface at one setting

On this Swift Lathe the 4-way turret is mounted on hydraulic copying slides which provide copy forming motion to the surfacing slide and the complete form of the surface can be copied at one setting. The basis of this machine is the Swift 14SB Lathe with swing over bed 28 in. and maximum distance between chuck and turret is in this case 2 ft. 6 in. A range of 18 spindle speeds is provided from 9 to 420 r.p.m. with 9 sliding feeds from 12 to 450 c.p.i.

Quick power traverse to the surfacing motion is fitted.

Write for full details of this special Swift Lathe and Surfacing and Boring Lathes of other capacities in which you may be interested. There is a range for diameters up to 5 ft. in gap and 3 ft. 3 in. swing over bed.

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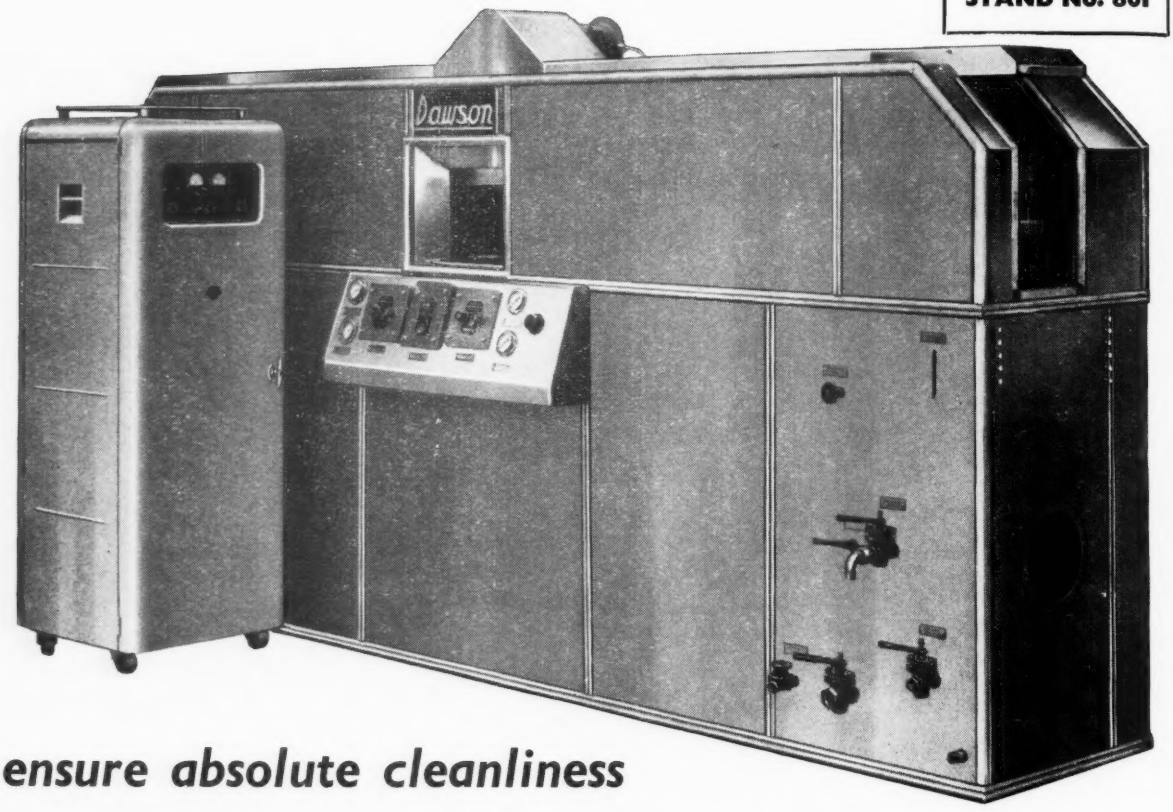
ULTRASONIC MACHINES

SEE THE LOW FREQUENCY TYPE AT THE INSTRUMENTS ELECTRONICS & AUTOMATION EXHIBITION OLYMPIA 7 - 17 MAY STAND No. 801



ensure absolute cleanliness of precision components, automatically

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Dawson machines have now been developed to work in conjunction with generators operating at frequencies of 20 K/cs and also at 1 megacycle.

The High Frequency Unit, illustrated above, has an average loading on the transducer array of 10 watts per sq. c.m., and a component in the field of activity is bombarded at the rate of one million times a second by minute particles of the cleaning fluid.

The Low Frequency method of Ultrasonic cleaning operates on a different principle known as cavitation, which gives the effect of a powerful scrubbing action on components immersed in the liquid.

Each method of Ultrasonic cleaning is specially applicable to certain types of component and varying deposits to be cleaned off.

Write today for further details of this new equipment, which offers exceptional opportunities for fast automatic cleaning of difficult small parts.

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H.F.166

Game to the very end!

No tool in the shop would satisfactorily plane a drop-stamp hammer block (2¼ ft. square by 18 ins. deep, of 80/90 ton nickel-chromium-molybdenum-steel) until they took this Allenite "Plowrake" tool off machining steel castings and put it to work on the die block. This did the job nicely, at 85 ft. per minute, ¼ in. to ½ in. cut, $\frac{1}{32}$ in. feed, removing metal from all sides of the block, the bulk of it to form the inverted-vee location slide. ★

At no time was the tool seriously chipped, and as the operator said, "it seemed to wear nicely"; and so it did, right to the very last bit, as will be seen by comparison with a new tool of the same size.

Allenite "Plowrake" Tools are achieving remarkable performances in the high speed heavy planing of steel forgings, castings, etc., especially those having interrupted surfaces. This is what this new Allenite tool can do when planing steel. We supply, however, many standard Allenite tools for turning, shaping, roughing, etc.

★ *Facts by courtesy of Jackstead Engineering Co. Ltd., Sheffield, who specialise in this kind of machining.*

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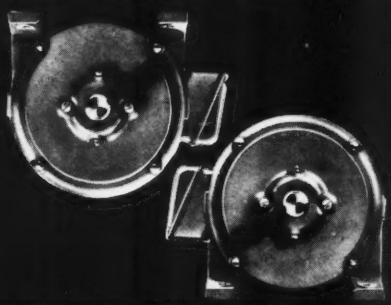
the NEW Newman DRIP-PROOF

built to Draft BS CW (ELE) 6246 with class 'E' insulation

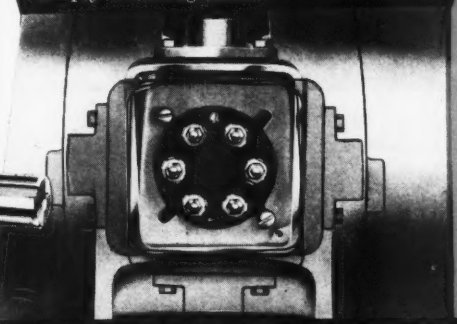
VERSATILITY

By the removal of four retaining nuts the end brackets may be removed and turned through 180° for ceiling mounting with the air vents at the top —installation is quick and easy!



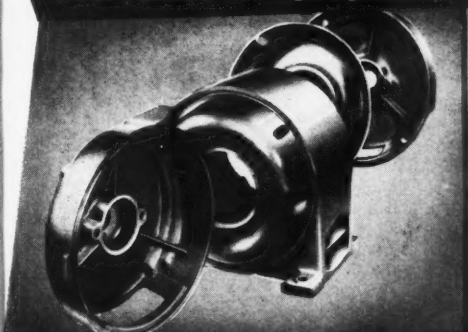
QUICK INSTALLATION

Two screws secure terminal box lid; cable outlet can be set four positions at 90°; screwed conduit gland is detachable; terminal markings permanently engraved. Terminal box can be positioned on opposite side of motor by simply reversing stator.



MORE COMPLETE PROTECTION

Skilful design of inlet and outlet air vents and air deflectors (which serve the dual purpose of creating efficient air flow and doubly protecting the end windings) give the new motor more complete protection.



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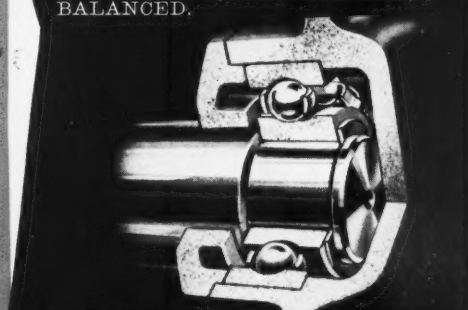
Carefully chosen ball bearings of standard metric size are lightly preloaded to ensure minimum noise and smooth running. EVERY ROTOR IS DYNAMICALLY BALANCED.



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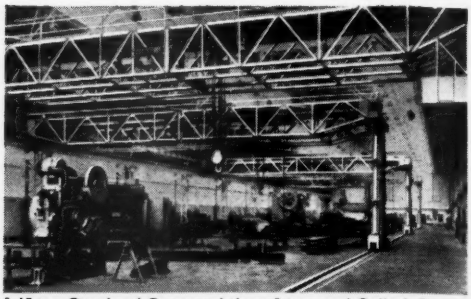
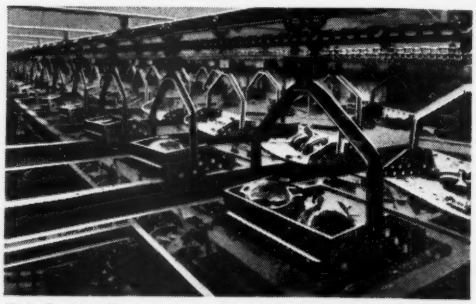
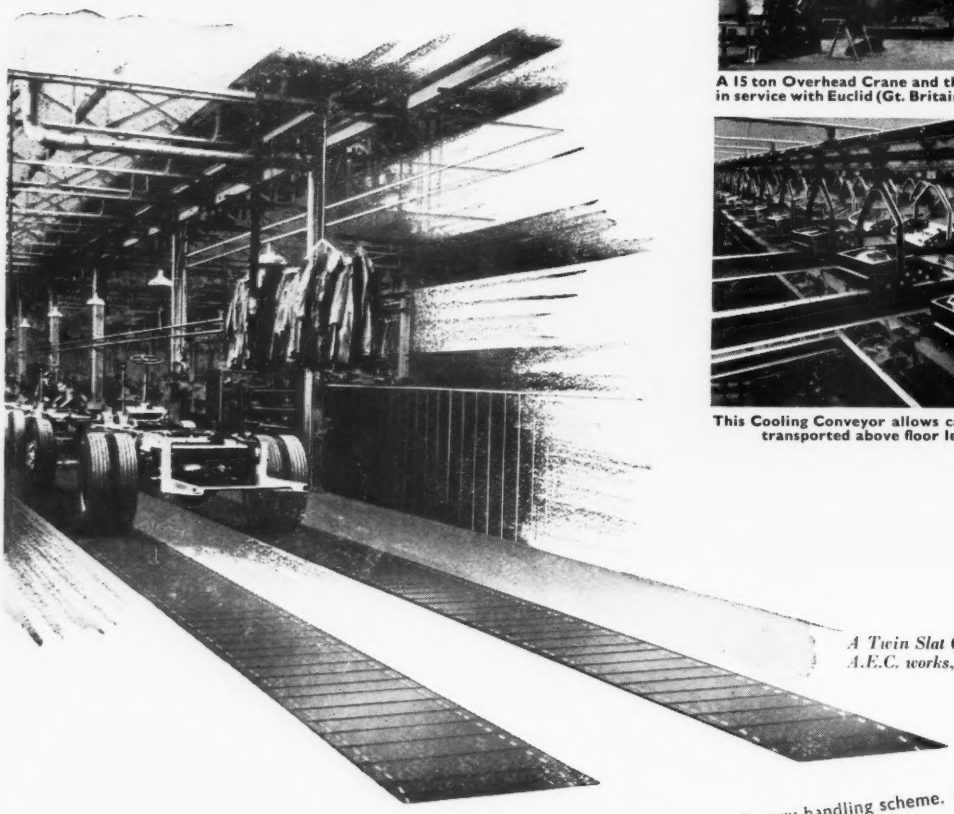
In the air or on the ground



A 15 ton Overhead Crane and three 5 ton semi-Goliath Cranes in service with Euclid (Gt. Britain) Ltd., Newhouse, Lanarkshire



This Cooling Conveyor allows castings to cool while they are transported above floor level to the fettling shop



A Twin Slat Conveyor in operation at the A.E.C. works, Southall, Middlesex

Paterson Hughes mechanical handling equipment provides a complete system for smoother, quicker production. Nearly 30 years' experience in handling goods and materials of all types has resulted in the design and manufacture of the most efficient handling equipment in service today.

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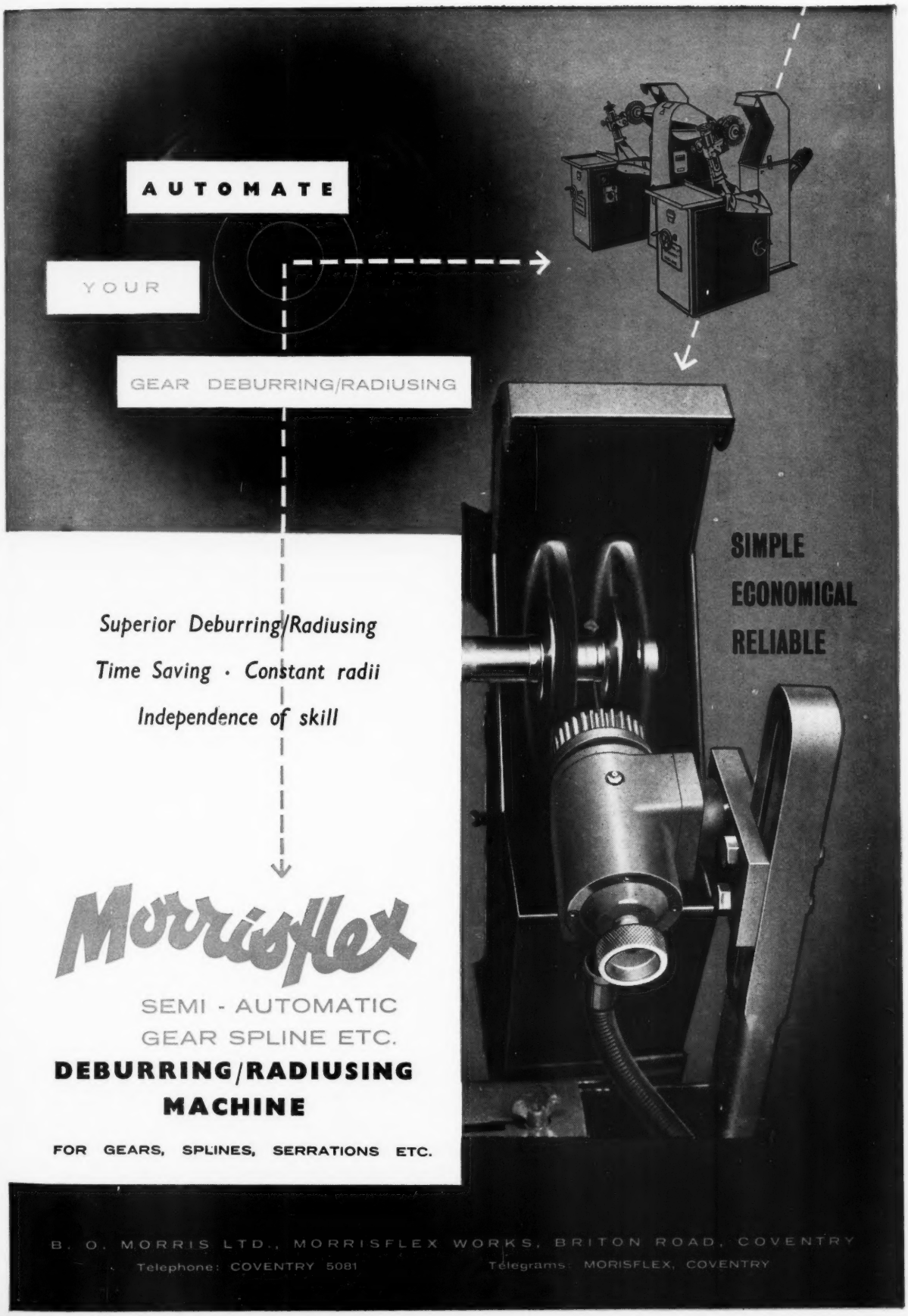

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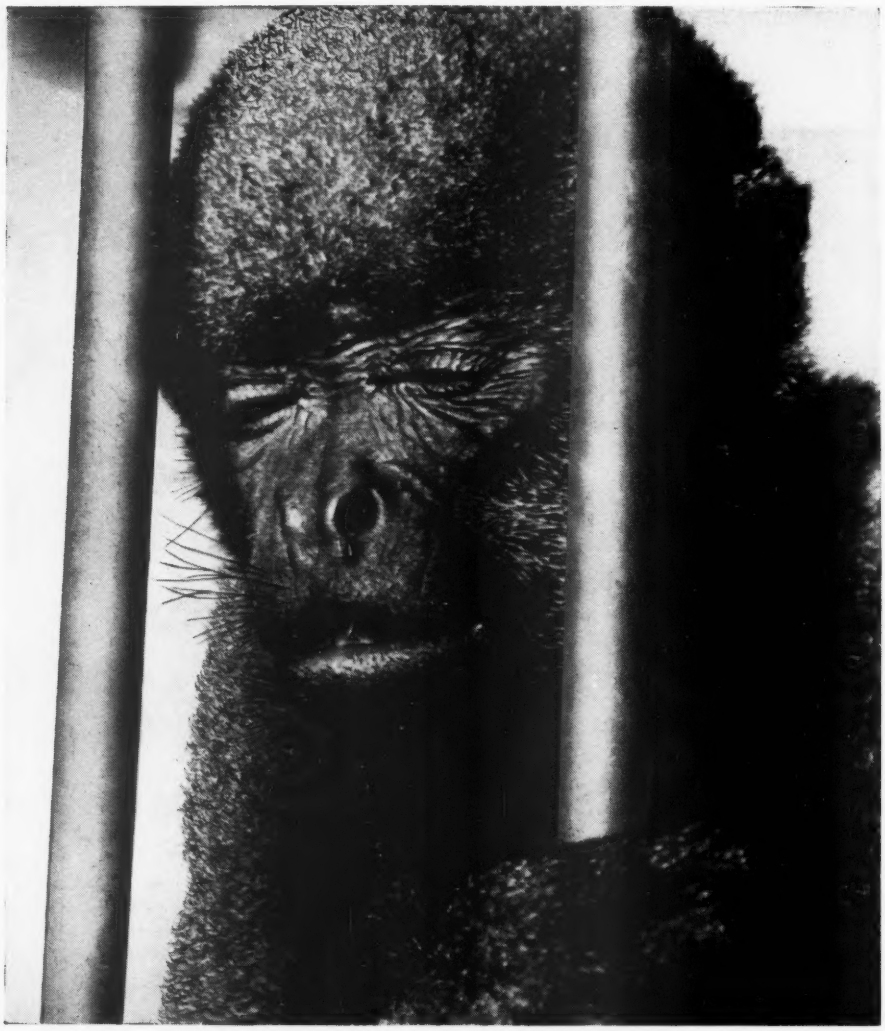
Wrought Iron puts Safety in the Forefront



Unlike this obvious source of menace, many modern dangers are unseen. Hundreds of lives may hang on the ability of cable chains, couplings, cage and lifting gear and similar equipment to withstand incessant strain. *That is why Wrought Iron should always be specified for such equipment wherever human safety is at stake.*

Wrought Iron stands alone in its resistance to shock and over-strain, to rust and corrosion. Moreover, the very nature of Wrought Iron makes it highly suitable for welding. It is, therefore, in the long run—especially under exposed or wet conditions—both safer and more economical than mild steel for many purposes. At the Midland Iron Works we produce Wrought Iron in strips and bars, in a variety of sections, for a multiplicity of uses. We will gladly send you a free copy of our Section Book, and our technical representative is at your service for consultation on any Wrought Iron application.

Here is an example of the ductility of Wrought Iron. A soft, malleable iron that can be forged and hammered at high temperatures, it is essentially fibrous in its structure. This characteristic enables it to recover from sudden shock and, when overstrained, to give warning of impending failure by visible elongation.

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you can safely use Wrought Iron for a host of purposes

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For faithful reproduction of intricate profiles

automatically

from simple quickly produced templates

and reversion to manual control at will

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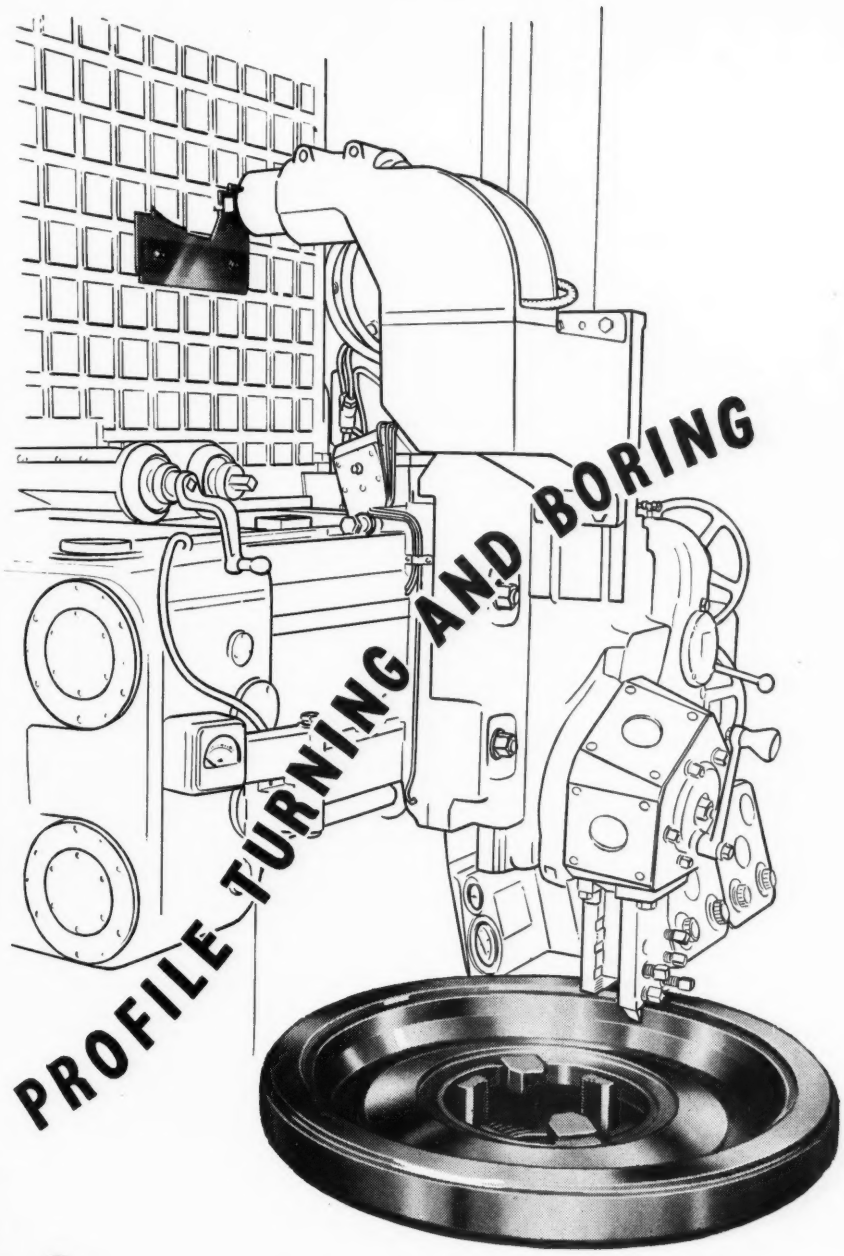
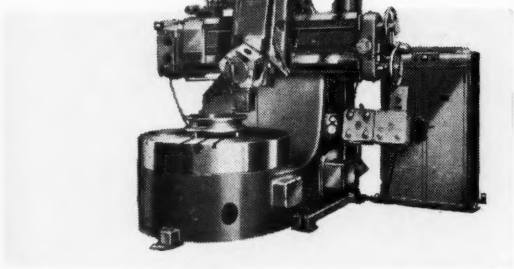
PROFILE TURNING AND BORING





The application of electronic profile turning and boring equipment to the Webster and Bennett range of vertical boring mills opens up a wide new field of uses for these versatile machines. Although specifically designed for copying applications, when not required the electronic control may be disengaged and the machine operated as a standard machine under mechanical control.

Machine capacities range from 36″ to 72″ chuck diameter, and machines can be supplied with or without the electronic profiling equipment.

WEBSTER & BENNETT LTD., COVENTRY, ENGLAND

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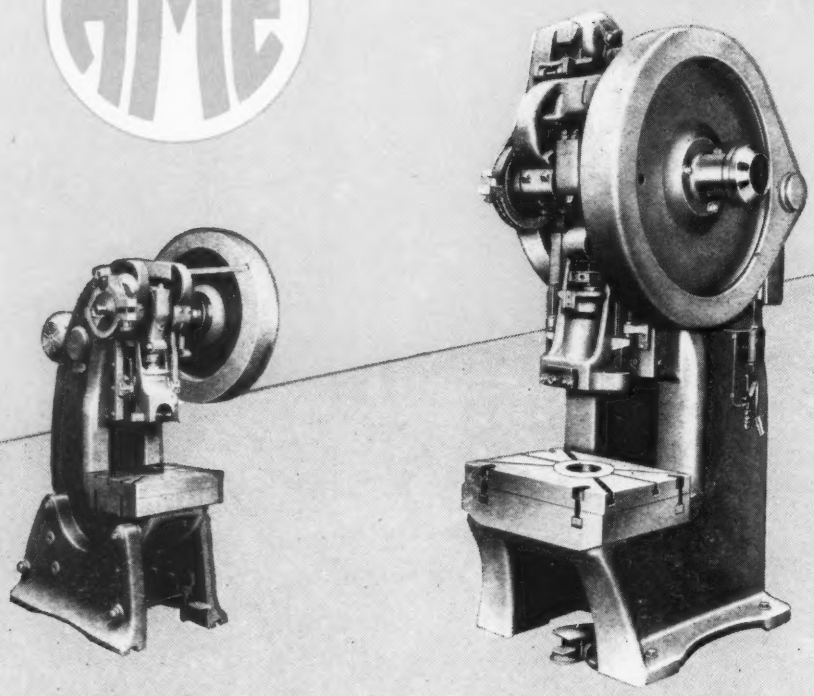

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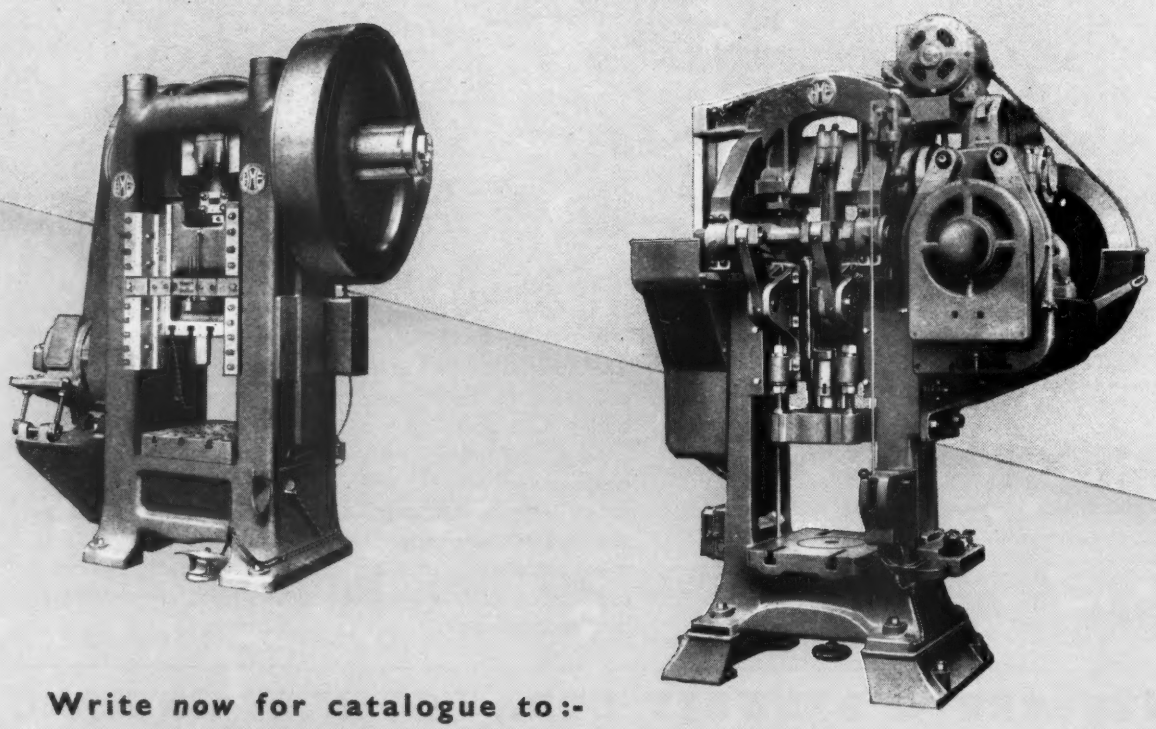

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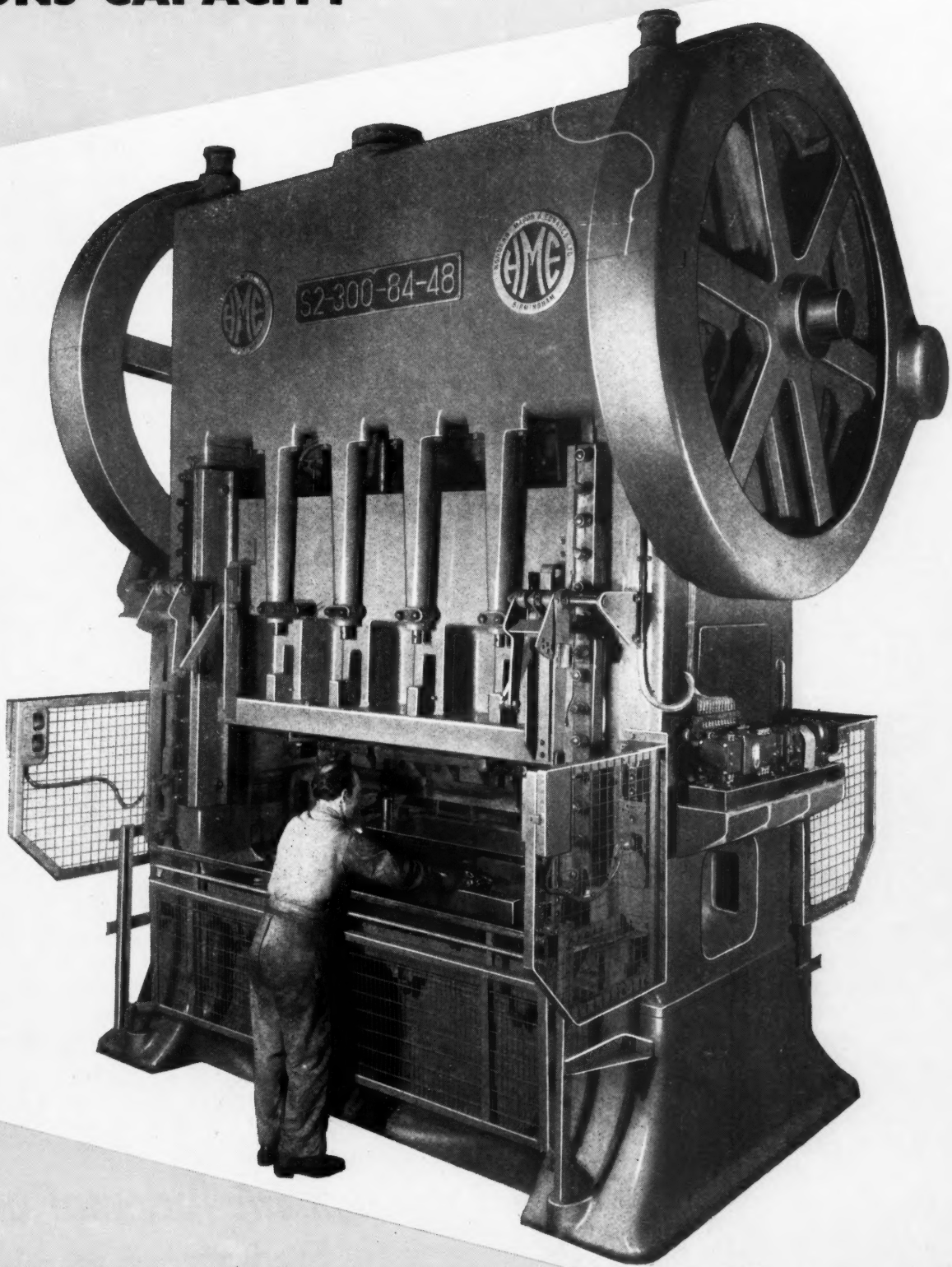

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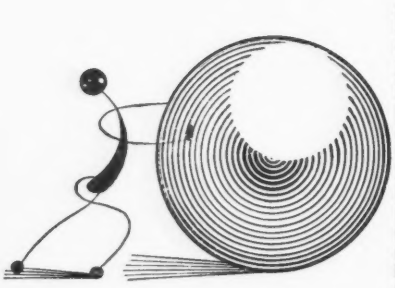

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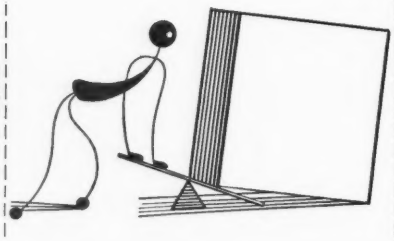

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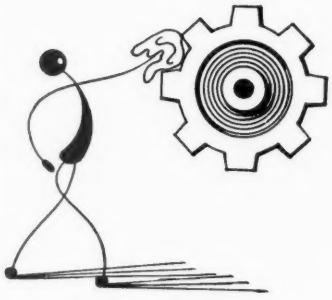

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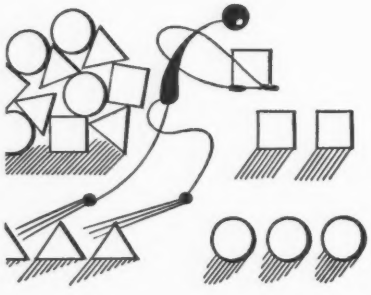

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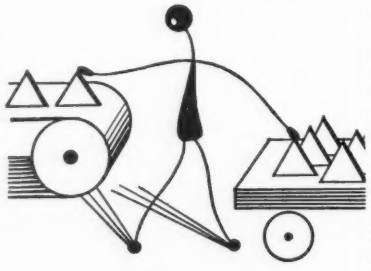

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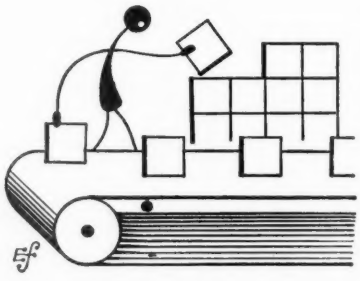

...feed parts

. . . reach, grasp, carry, lift, turn, clamp and tilt. Hymatic Automation is thus invaluable in the automatic and selective transfer of parts from one process to the next, from conveyor to conveyor; in unloading one machine and loading the next; or in quick clamping to hold parts during machining. All designs of Hymatic Automation are made for specific tasks but are readily adaptable to others.

Hymatic Automation covers the whole problem—the design of the circuit—the machine—the means of control.

Hymatic Automation, therefore, has a place in your programme of productivity improvement.

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THE HYMATIC ENGINEERING CO. LTD. DEPT. HA.84 · REDDITCH · WORCS.

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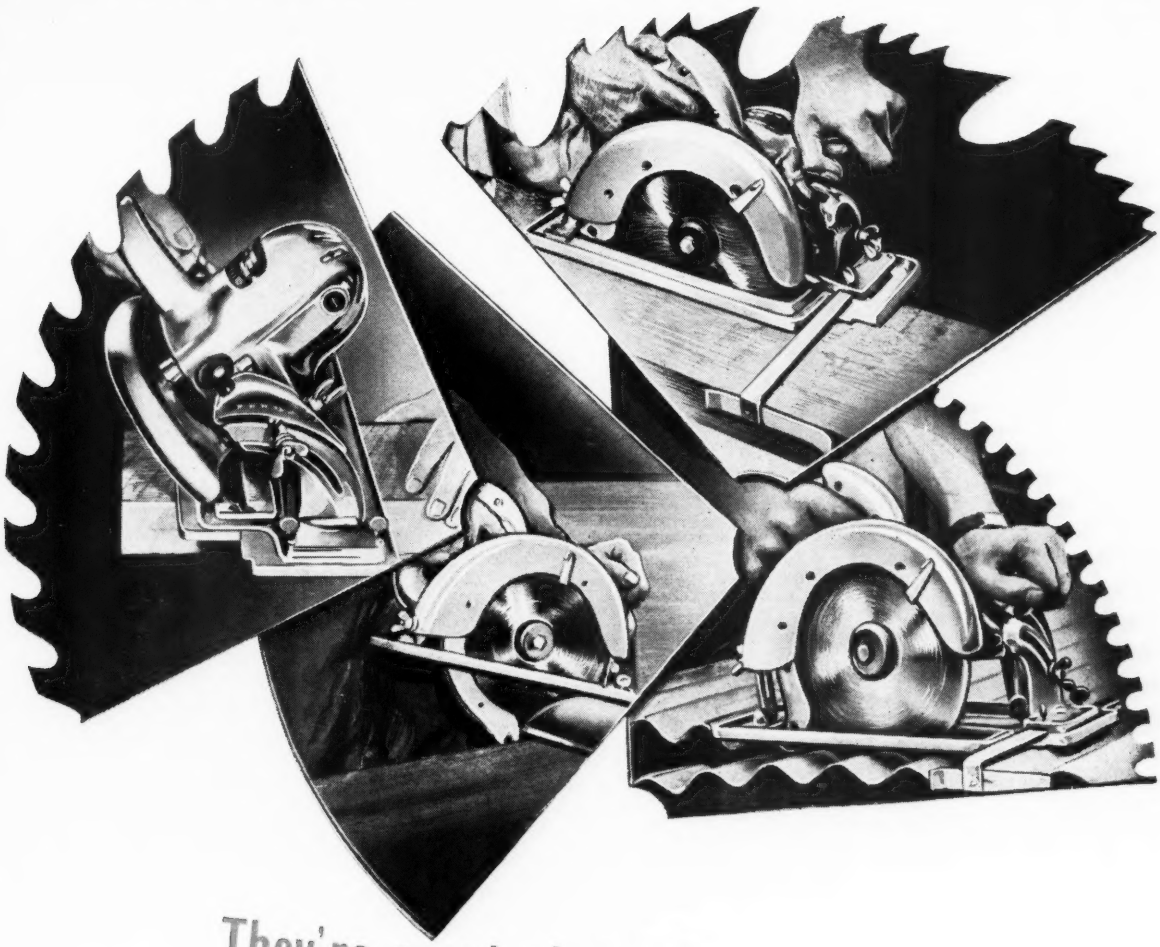

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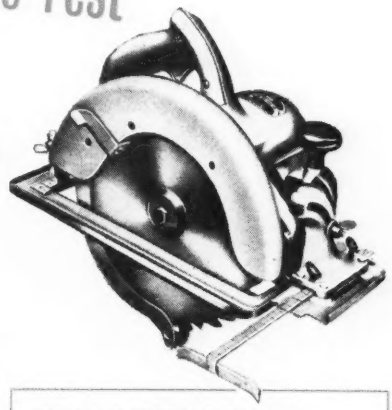

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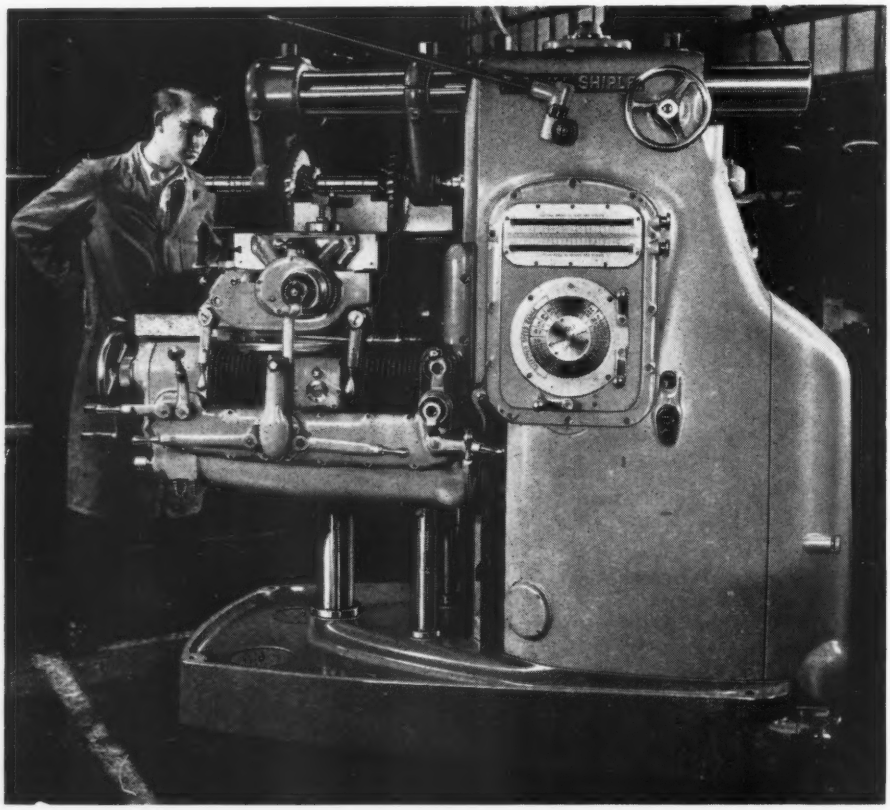

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Positive Backlash Eliminator	Yes			
Suitable for Tungsten Carbide Milling	Yes			
Suitable for High Rake Milling	Yes			
Duplicated controls and LOCKS at front and rear	Yes			
Every Final Drive Gear bigger than the Cutter it drives	Yes			
Table Size	60" x 14"			
Feeds	Infinitely variable— ½" — 64" per minute			
No. and Range of Speeds	24 from 23 — 1250			
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Automatic Slide & Screw Lubrication	Yes			
Knee Width	19"			
Built in Fly Wheel	Yes			
No. of Spindle Bearings	5			

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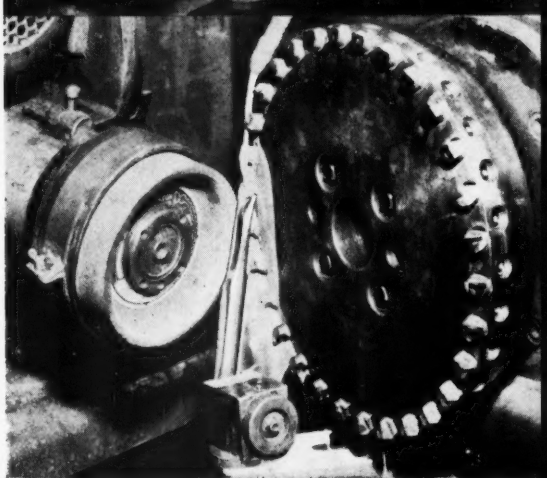
make a valuable cost cutting contribution to Carbide Grinding



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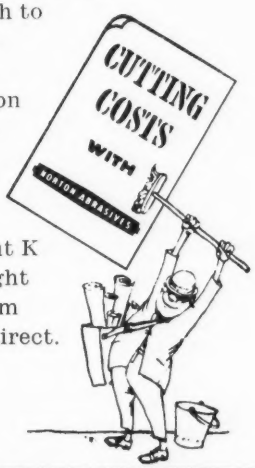

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"GALTONA-O.K." Serrated Blade Cutters have proved themselves supreme in equating the rigidity and power of the machine in terms of heavy metal removal, high finish, fine limits and complete dependability.

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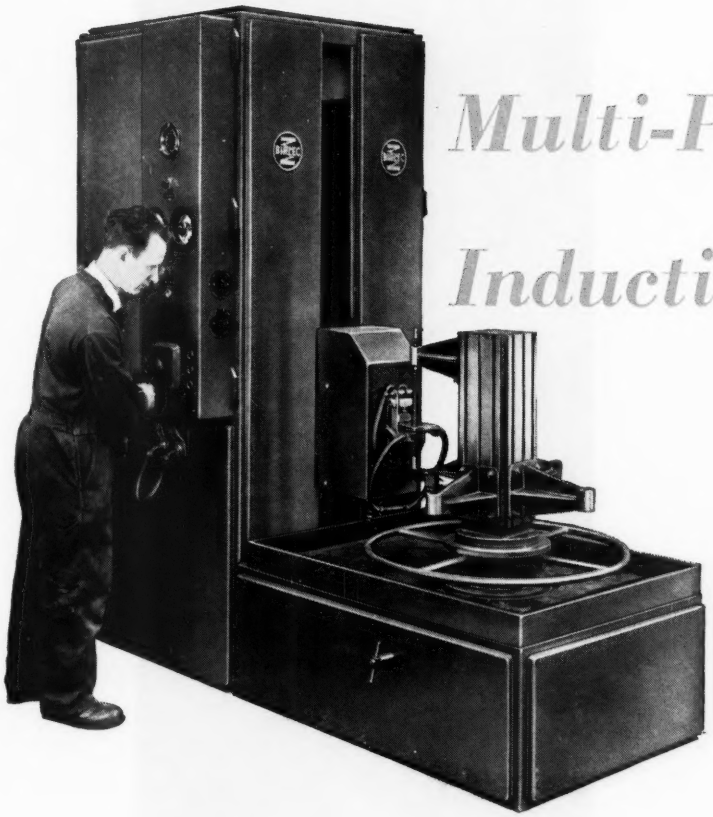

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Permitting rapid interchanging of inductors and instantaneous positioning of its four-station work support head, this new Birlec induction heating machine can be tooled for a number of varied jobs without individual time wasting re-adjustment. Equipped with a power traversed heating head, the machine can perform in quick succession a wide variety of single shot or progressive heating operations including hardening, brazing, and soldering. One typical installation hardens 24 different gear box components. Birlec publication number 140 gives further information.

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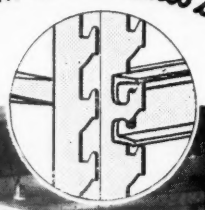
Pallet Racks

The Rack with the Boltless shelf!



Eliminated ⅔ of former handlings

These *Before* and *After* photos show how time in handling of materials was cut by two-thirds in this plant—and how storage capacity was increased by 300%. These racks can do the same for your plant. Investigate today!

Increased capacity for work in process storage by

300%

COURTESY THE WESTINGHOUSE BRAKE AND SIGNAL CO. LTD., CHIPPENHAM, ENGLAND

In this installation RACK Adjustable Pallet Racks made possible a major increase in output through providing quick availability of parts. Savings in handling costs and in storage space were substantial. Overall efficiency of Department increased considerably with introduction of *"Good Housekeeping"* Policy, and with flexibility of the RACK Material Handling Equipment.

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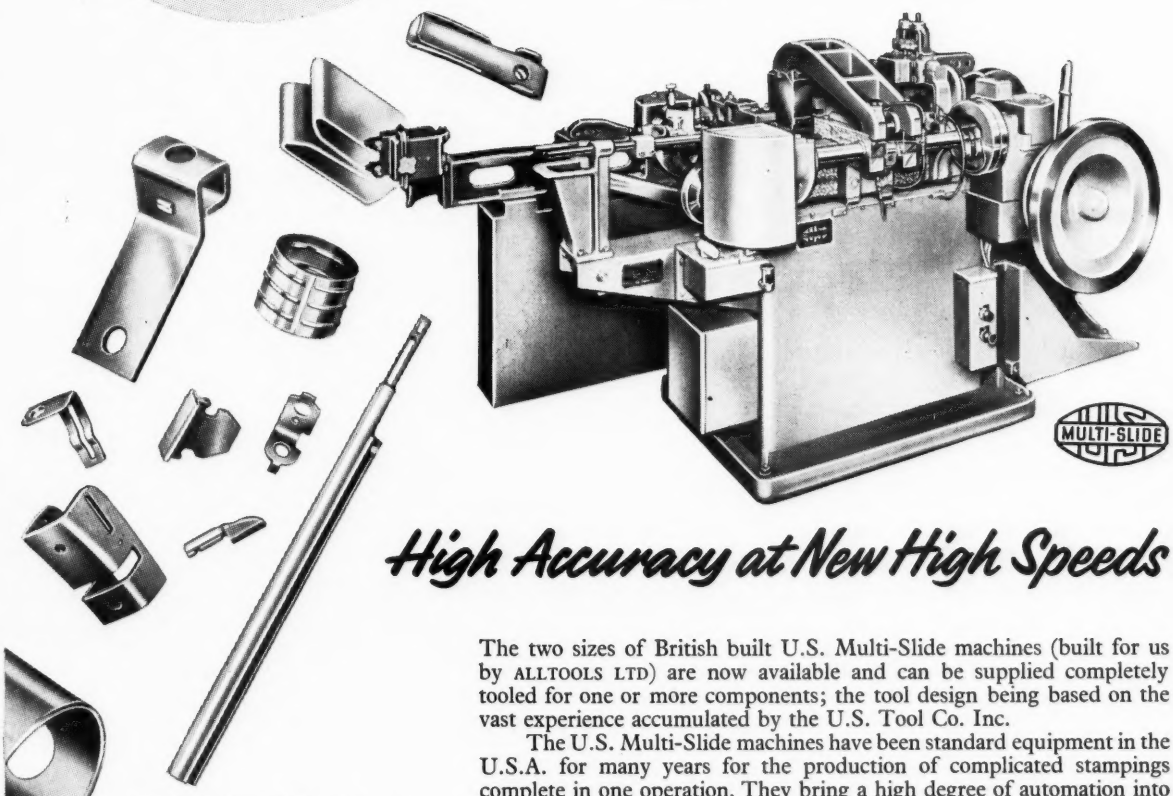
The U.S. MULTI-SLIDE *now British Built*

INTRICATE STAMPINGS IN (ONE) OPERATION



High Accuracy at New High Speeds

The two sizes of British built U.S. Multi-Slide machines (built for us by ALLTOOLS LTD) are now available and can be supplied completely tooled for one or more components; the tool design being based on the vast experience accumulated by the U.S. Tool Co. Inc.

The U.S. Multi-Slide machines have been standard equipment in the U.S.A. for many years for the production of complicated stampings complete in one operation. They bring a high degree of automation into every press shop and are indispensable for the economic and accurate production of many components used in Automobiles, Office machinery, Radio & Radio Valves, Television Receivers, Electrical and Electronic devices, Hardware, Locks, Razors, Book-bindings, Household utensils, Bearings etc.

Tools are built up in sections and placed in die sets, permitting easy adjustment to suit part modifications, low tool cost maintenance, and easy change-over from one job to another.

Send for illustrated leaflets, *also ask to see film* showing operation of the machine and examples of components produced. Our Sales Engineer will be pleased to call on request.

Brief Specifications

Machine Model	28	35
Length of Feed	8"	12½"
Maximum Width of Stock	1½"	3"
Maximum Thickness of Stock	1/16"	3/32"

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US. 3

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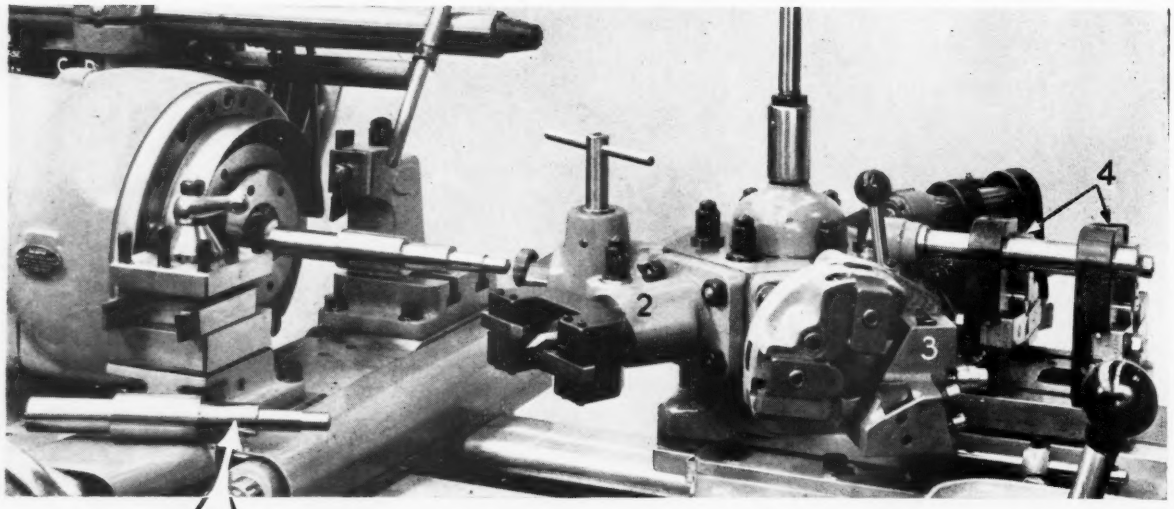
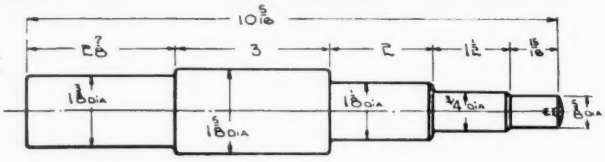


CENTRE SHAFT



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40 Ton Tensile Steel

Tungsten Carbide and
H.S. Steel Cutting Tools

No. 7 CAPSTAN LATHE

FITTED WITH 2 IN. AIR AUTO. BAR
CHUCK AND AIR BAR FEED

DESCRIPTION OF OPERATION	Tool Position		Spindle Speed R.P.M.	Surface Speed Ft. per Min.	Feed Cuts per inch
	Hex. Turret	Cross-slide			
1. Feed to Stop and Centre Drill	1	—	1000	—	Hand
2. Start Turn 1⅛" and ¾" dia.	2	—	1000	460	Hand
3. Support and Double Turn 1⅝" and 1⅜" dias.	2	Front	1000	460	152
4. Tangential Turn 1⅛" dia.	3	—	1000	460	133
5. Multi Turn ¾" and ⅝" dias.	4	—	1000	295	133
6. Radius, Chamfer and End (Multi-Toolholder)	5	—	1000	425	Hand
7. Support and Part Off	6	Rear	1000	360	Hand

Floor-to Floor Time: 3¼ mins. each

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BRITISH MACHINE TOOL MAKERS ASSOC. LTD

W653

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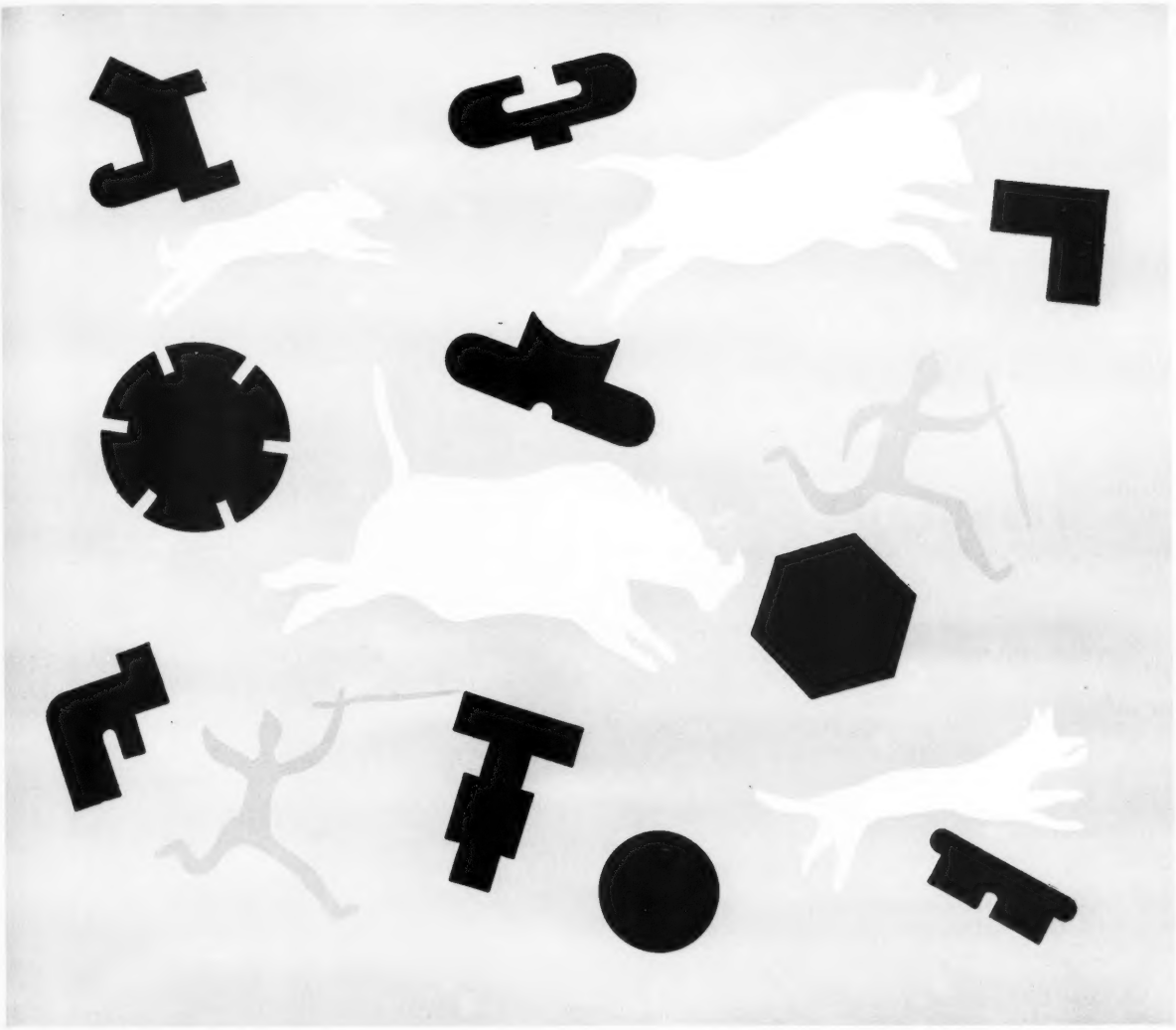

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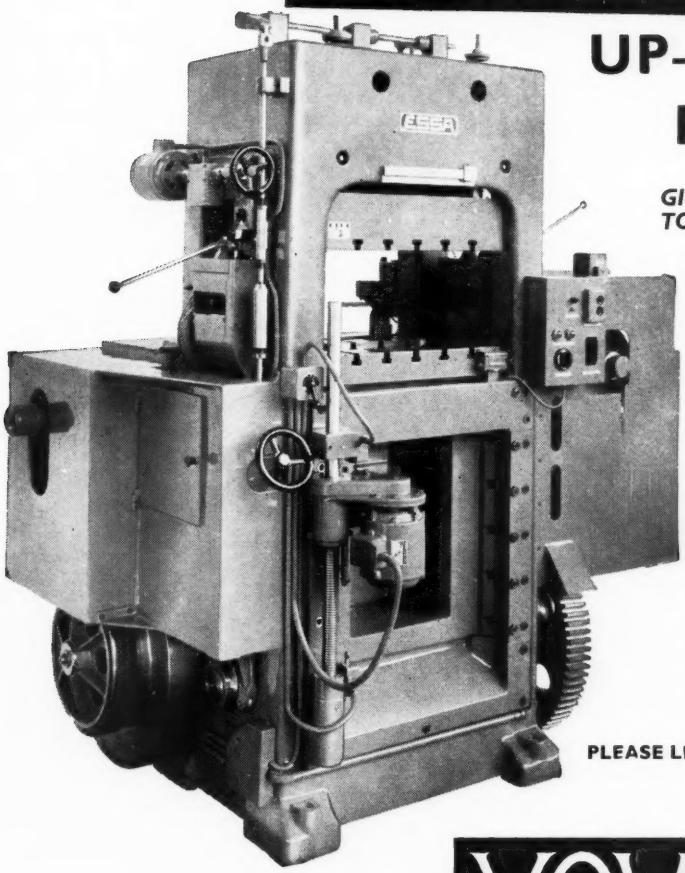

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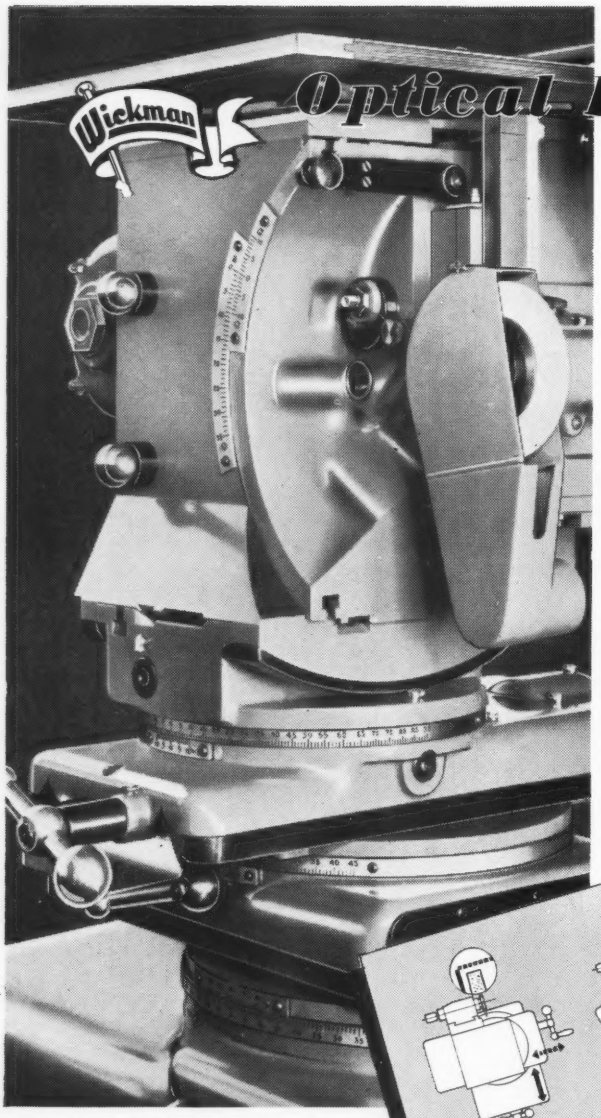


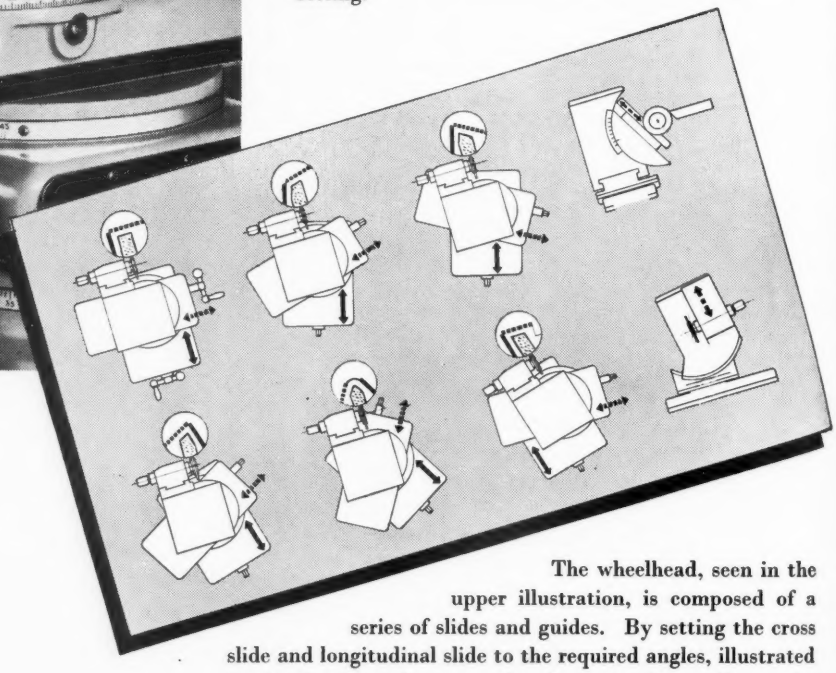
Optical Profile Grinding

Traverse-ground angles simplify form production

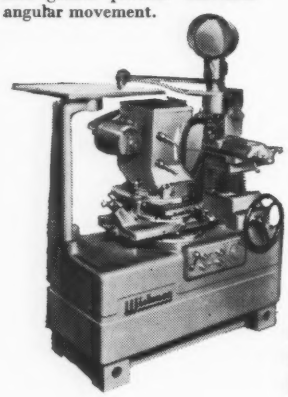
Although the principle of operation on this machine is based on a pantograph controlled optical system for the production of irregular contours, when the required profile includes angular forms, these can be more simply produced by utilising the multiple slide arrangement of the wheelhead (see sketches).

This is composed of a series of slides and guides and by setting the cross slide and longitudinal slide to the angles required angular forms can be ground by traversing, using the pantograph and microscope at the end of the angle for checking purposes.

Clearance angles can also be ground as part of this forming operation without affecting work setting.



The wheelhead. Multiple slides and guides provide universal angular movement.



WORK RANGE

Maximum width	5⅞″
Depth of Form	2½″
Maximum thickness	3″

The wheelhead, seen in the upper illustration, is composed of a series of slides and guides. By setting the cross slide and longitudinal slide to the required angles, illustrated in the sketches above, regular forms in the profile can be ground by direct traverse, using the pantograph and microscope only at the end of the angle for checking purposes.

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The Institution of Production Engineers Journal
VOL. 36 NO. 5 MAY, 1957

The Challenge of the Age

by WALTER SCOTT.

Governing Director, W. D. Scott & Company Pty. Ltd., North Sydney, N.S.W.; Federal and N.S.W. President, Australian Institute of Management.

In the September, 1956, *Journal there appeared a leading article written by the President of the Institution, Mr. E. W. Hancock, M.B.E., and entitled "The Challenge of the Age". In his article, Mr. Hancock emphasised the unique responsibility of the production engineer in making readily available to the nation the benefits resulting from technological advances. The demands of increased competition in world markets and of far-reaching new developments and processes constitute a further and growing challenge and underline the importance of the production engineer's role in industry.*

In order that members may be kept informed of how this universal challenge is being met elsewhere, leading industrialists in a number of countries are being invited to contribute articles to a series, under a general title as above. In this, the first of the series, Mr. Walter Scott reviews the position in Australia.

IN thinking of all aspects of Australian economic life, it is easy to think of several factors which could, in themselves, constitute the Challenge of the Age. The first thought may lead to the Challenge of Change. The tempo of change affecting business has never been faster nor more intense than it is today. Nothing is surer than the continual change in conditions which faces industry through new materials, new resources, new machines, new competition and new techniques.

The well-organised business is overtaking the old leaders and the stability of every company which fails to adjust itself is threatened by the impact of advancing technological change. As has been said by Don. F. Copell — "Remember that no war, no strike, no depression can so completely and irrevocably destroy an established business as new and better methods in the hands of an enlightened competitor."

Science has it in its power today to render obsolete any product almost overnight. Machinery and manufacturing methods must therefore be modernised ; new products must take the place of those that have lost their appeal and markets and marketing methods adjusted accordingly. To some, perhaps to many, there will be the question of diversification as a hedge against recession, to even out seasonal ups and downs, to give a company room to grow, or perhaps to make up for the cyclical swings of the market.

There is the impact of automation and all that it means — not, mark you, automation only for the big plant, because automated processes are also being designed for smaller plants.

There is the field of electronics and all that it can bring both in the factory and the office. It is already commencing to revolutionise office procedures and in some ways automation in the office is moving faster than it is in the factory.

Then there is the impact of atomic energy, with the use of radioisotopes beginning to affect industry after industry. Well may Sir Winston Churchill say that nuclear power can unfold "a future beyond the present dreams of mankind."

All these things are looming large on the businessman's horizon, and their shadows are shaping more clearly the march of coming events. Their impact will be felt in the factory, in the office, in distribution, in transport, in agriculture, in health and in the home. Furthermore, they will provide problems — great problems, such as the following:-

1. Competition — on an international, inter-industry and industry basis.
2. Understanding, interpreting and coping with national economic factors.
3. Understanding the new technologies and their impact (as, for example, that automation in its final analysis creates more of a marketing than a production problem).
4. Learning how to make decisions for a longer future than at present.
5. Learning better how to build the most precious of managerial resources (and the most difficult), organisation-building.
6. Comprehension of the forthcoming social effects.
7. Helping to sponsor a tremendous increase in the number of trained and educated people in business.
8. Learning more thoroughly and more consistently the science of management.

Yes, there is a great challenge in change. But perhaps it is not here that we must look for the Challenge of the Age. Maybe it is to be found in a related yet different sphere. Perhaps it is the Challenge of Productivity.

The keystone of progress by every industrialised country is productivity. The major aim of every semi-developed country in the world is to increase its productivity. The under-developed countries of the world are awakening to a productivity call, because they see in it the one way whereby their economic shackles can be loosed. Democracy, socialism, totalitarianism — each is dependent upon productivity for its very progress.

Productivity is of the greatest importance to Australia, because:-

1. It shares with other countries the essential of raising and keeping high the standard of living, as the following considerations indicate:
   (a) All democratic countries have at least partly committed themselves to a welfare state, which means that the financial burdens of social security have grown rapidly and must be borne by the productive section of the community. The burden is reduced as productivity increases.
   (b) All democratic countries have tended toward equality of income, and this could mean equal misery or equal prosperity, according to productivity.
   (c) There is a constant pressure on the Australian economy by basic wage adjustments, and the spiral thus set up has meant major headaches for all governments. Productivity can help to offset inflation arising from increased wages.
   (d) There is a wide margin before any situation wherein productivity means over-production is even approached. It is rather significant that the great examples of over-production at the moment occur, not in the industrial field, but in agriculture, as typified by U.S. surplus wheat and other commodities.

(e) In a young and developing country such as Australia there are tremendous requirements of transport, education, hospitals and cultural activities. Capital and productivity can decide whether a nation must make inadequate progress in these fields, speed up progress at the expense of consumer demand, or make adequate progress without making undue inroads into consumer demand.

(f) The knowledge that certain products can be produced often promotes a desire for them on the part of the public. These desires can become insistent, and can best be satisfied through increases in productivity.

2. It is by far the most desirable way of increasing production. There are five ways in which this can be done :

(a) By people working harder.

(b) By people working longer hours.

(c) By more people working.

(d) By people starting to work younger and/or retiring later.

(e) By working more efficiently — that is, by increased productivity.

Over the years, the tendency — at least in the Western World — has been to work less hard, to reduce fatigue, and to make work less exacting. The hours of work have been steadily decreasing, particularly since the turn of the century, and the standard work week is now shorter than it has ever previously been.

It is in the field of working more efficiently that the real progress has been made over the years and it is true, at least as a generalisation, to say that better methods have been the major agency for increasing production and that, within the category of better methods, better machines have made the greatest contribution. More people are at work in Australia than ever before, though it must be remembered that the extra numbers make greater demands in capital and consumer goods. It is this combination of conditions that Australia really wants — more people working, with ever increasing productivity.

3. Productivity can be of greatest help in defence. History allows no challenge to this statement. Every modern war has largely depended upon industrial output and today the dependence is complete. The means of war can be forged only in the factory and productivity becomes vital in wartime. There is, however, a further aspect.

Defence requirements are ever present. Increased productivity is the equivalent of saving the manpower that would normally be represented by the increased production. Defence can then be taken care of more easily with a lessened adverse effect upon the standard of living. It becomes possible to get both guns and butter.

4. Productivity is one of the biggest single factors in securing reduction in or preserving costs. It is in every sense vital to Australia as indeed it is to every industrialised country in this highly competitive age.

Yet it is probable that the Challenge of the Age is not to be found in Change, nor even in Productivity, but perhaps in People. For in all this world of change, *people* have not changed. True, they may appear to have changed. They may seemingly be interested in somewhat different things. They may regard yesterday's luxuries as today's necessities. They may be prepared to take part of today's improvement in the standard of living by extra leisure rather than by extra pay ; but in the final analysis, people, the real people, have not changed. There are the same differences in intellect and in temperament ; in motivations and in loyalties. There are the same mistrusts and suspicions ; the same mixtures of kindness and hurtfulness ; the same generosities and the same greeds.

This then is where the Challenge of the Age is really to be found — the necessity to pursue resolutely and determinedly the added knowledge, the better practices, the improved methods, techniques and skills which the scientist and the engineer will unfold, and to harmonise these with the aspirations of human beings so that there will be a competitive lead in one's industry and social, moral and industrial progress in one's country. And this task of harmonising is the task of management. So, in the final analysis, the Challenge of the Age is Leadership and that challenge is directed to management to provide that Leadership.

This is no easy task. To accomplish it is to succeed in the future where there has been so much of failure in the past. Somehow the touchstone that would have opened up this success has eluded us and that has, in truth, been a great loss — not Australia's loss ; not England's loss ; not a loss of the British Commonwealth, but a loss in which the whole world has shared. Well might Elton Mayo have written in "The Social Problems of an Industrial Civilisation" —

> "One might say of recent history that each successive decade has brought a competent observer to warn us of our failure to study man, to consider the effect upon him of all this progress. Such warnings, Cassandra-like, have passed unheeded ; it has taken major tragedy — catastrophe indeed — to call our attention to the realities of the human scene."

To what catastrophe did Mayo refer ?

> "The consequences for society of the unbalance between the development of technical and of social skill have been disastrous. If our social skills had advanced step by step with our technical skills, there would not have been another European war ; this is my recurrent theme."

The answer to this challenge lies in three directions. Firstly, in the fact that management itself must increase its own competence and vision but, above all, its own characteristics and attributes so that it can at last solve this problem which dwarfs all other problems. To accomplish this,

1. Management must have the ability to inspire men to work with faith in the future and faith in itself — that is the very essence of leadership. If there is failure in this, leadership can be written out of the definition of management.

2. Management must have an unswerving belief in honesty, integrity and loyalty. All other virtues fail without these. They should be the very cornerstones of management ideals.

3. Management must have determination and perseverance. Obstacles there will be and plenty ; set-backs, many and often, and there will be an urge to take the easier road. Management is, however, dishonest with itself if it leaves to chance what it can influence for good.

4. Management must have humility. This is a virtue which, above all others, can save it in times of trial and error. It must realise that as a class, it is as capable of making errors as is any other class ; so humility is a saving virtue.

5. Management must have initiative and be creative — initiative to take advantage of changing situations and creative that it may create them.

Secondly, management cannot succeed without recognising the part which education must play in the future. Not without cause is the word education linked with "*educare*" — to lead forth. Its roots signify the capacity to draw forth and to develop — which comprises the fundamentals of leadership. Only by education can management duplicate itself throughout its organisation ; only by education can management give to employees that knowledge by which they can successfully and enthusiastically tackle the jobs at hand.

Thirdly, and most important of all, in all this changing world, in this world where the scientist and the engineer are irrevocably changing much of the shape of things that were and unfolding the shape of things that will be, it must be management's task to mould these for good. Where does the engineer fit in to this pattern ? One's mind goes back to some of the great engineers of the past. Examination of their lives will show how much they conformed to the pattern outlined here. They were great leaders, not necessarily because they had great ability, but because they had so many of the qualities — the essential qualities — that have been described. The great pioneers in management were largely engineers — Taylor, Gilbreth, Gantt ; they made the profession of engineering part of the profession of management. In many ways, the outlook of the engineer is and should be in large measure the outlook of management.

Undoubtedly these things spell problems — great problems — but the answer is to be found in the very nature of problems themselves. There is nothing to be afraid of in problems. They do not necessarily mean sickness, but are much more the essence of life which is always struggle and challenge. As R. J. Blakeley said three years ago to the Adult Education Association in U.S.A. : " Problems are of the nature of digestion, not of the nature of ulcers ; the nature of breathing, not of the nature of bleeding. It is in the way problems are dealt with that there is sickness or health, in the difference between being overwhelmed by a problem and transforming it into another at a higher level. This is a harsh doctrine for the weary but an invigorating one for the strong, and no solution or transformation can be hoped for when the assumption is that everyone is ill."

We are in the midst of vast business and social change, but they can give to us new concepts of the nature and capacities of man and the development of the forces of human aspiration — and note it well — our form of aspiration is Democracy.

As Abraham Lincoln once said, " The measure of a man is to be found in the problems that surround him."

As Abram T. Collier wrote in " Business Leadership and a Creative Society "—

> " Moreover it seems that a religious sense of wonder, humility and faith helps us to see the vision of a boundless future built by the inherent capacities of men from all walks of life and of all races, creeds, natures and backgrounds. It is a vision of co-operation, togetherness and sharing the great adventure. It is a vision of independence and courage that explores the far reaches of the universe and probes deep into the essence of what we call man. It is, in short, a vision of a changing, growing and infinitely exciting world which depends for its existence on the spirit that is not too sure it is right, on a deep-seated desire to open our minds and our hearts to the lives of others, on the practical sense of give-and-take, on our faith in the growth and development of ourselves and our fellow men."

Progress in material things and a continually expanding revelation translated into machines and controls may give to man many servants, but all these are without value unless management can give to the human resources of industry a new dignity, a new vision and a new horizon.

This, indeed, is the real Challenge of the Age.

THE 1956 GEORGE BRAY MEMORIAL LECTURE

THE SCOPE FOR OPERATIONAL RESEARCH IN INDUSTRY

by STAFFORD BEER

Presented to the Institution in the Sheffield City Memorial Hall, 18th March, 1957.

Mr. Beer was educated at Whitgift School and London University, where he read honours philosophy, with psychology. His education was interrupted by the Second World War, and he served in turn in the Royal Artillery, the Royal Fusiliers, and the Gurkhas. He returned from India, where he had become a Staff Captain, to work in personnel selection and the field of psychological research.

In 1948, Mr. Beer joined the United Steel Companies as a management apprentice. Drawing on his military experiences in research of an operational kind, which began in India, and on his studies in statistics, he began a systematic scientific study of the production control problem at the Companies' alloy and stainless steel-making branch, Samuel Fox & Co. Ltd. He made rapid progress and was eventually appointed Production Controller. During this period he reorganised the whole function for which he was responsible, creating for it a completely new set of techniques based on operational research methods.

Throughout this time, Mr. Beer's own research continued; he came to recognise in cybernetics the practical synthesis of his work in metaphysics, logic, psychology, philosophy of science, statistics and industrial affairs. This cybernetic thinking had results which found incorporation in his techniques of production control. As a result, Mr. Beer has been appointed Head of a new Department of Operational Research and Cybernetics for United Steels as a whole, and has now begun to extend his work with a team of some 25 scientists.

Mr. Beer is a Fellow of six learned societies, and is active on a number of committees. He is a prolific writer and lecturer in his many spheres of interest, which extend to the arts, and international attention has been attracted to his work.

ORIENTATION

(a) The Field of Problems

A major tenet of the industrial philosophy which has led us into an era of higher productive efficiency has been the concept of "the division of labour". Inevitably, perhaps, this doctrine has been applied elsewhere than on the shop floor; in particular, it has been applied in the two fields which are the primary concern of this Paper: those of industrial management and scientific knowledge. Doubtless the division of labour in both cases has sponsored many gains; but I venture to suggest that the blessings have been mixed. There has been some loss in *unity of thought*, a commodity always highly prized by the thinkers of the world. This is, I propose, the deep-lying source of some lack of cohesion in industrial affairs.

Another basic source of difficulty is discernible: this is the conflict between tradition and new ideas. The strong sense of tradition in our country is one of its greatest strengths. But we are uneasily aware that new techniques sometimes demand the abdication of ruling notions; it is not by any means easy to decide at what point such contextual conditions have to be changed. The time does come when even the most respected, tried and honoured dogmas of both management and science become so affected by the principle of diminishing returns that they are no longer serviceable. Recognising this time is a most difficult task for both the manager and the scientist.

Whenever contemporary industrial problems are being discussed, there seems to be an all-too-ready response to semi-political arguments. The "we" and "they" (depending on one's point of view) of these arguments are the Scylla and Charybdis of straight thinking. Obviously there are problems of industrial relations, and I do not seek to minimise them. But I do believe in management's right to manage, and I conceive that in right management lie the answers to our *real* problems — those whose sources I have just mentioned. Some appear to think that the demand is for more aggressiveness; on the contrary, it has never been true that "might is right". The real need is for more science: we are concerned here with one of its manifestations.

(b) Is the World Flat?

It is as difficult to define the nature of our answer in operational research to these questions as it is difficult to define a spiral staircase in words. It is better to discuss the techniques we use and their applications, and to let an understanding of the work gradually emerge. I have so far implied that operational research is concerned to apply the methods of science to the problems of management: but one gathers that there is virtually no one in industry today who is not engaged in that task. Without attempting a definition, then, I will try to give a preliminary impression of our approach.

The area of industrial decision is in an incoherent state. It is neither a closed system nor a static one. Rather is it a dynamic interacting complex of variables, in which nothing is certain, nothing completely controllable, nothing quite describable nor fully understood. In this arena almost any question is likely to become a focus of opinion rather than knowledge, of prejudice rather than fact, of passion rather than logic. Operational research works to create a Still Centre in this turmoil. Our aim is just this: to lift controversy out of the arena of controversy.

Let us note the historicity of this process. The development of the technique of applying science to life is something separate from the development of science itself. It was once possible for highly intelligent men to argue bitterly on the question whether the world is flat. Eventually the application of scientific method produced an answer beyond cavil. Today there are many questions relying for solution on judgment, guesswork or prejudice to which scientific conclusions can be reached by developing this technique of *applying* science. The trouble is that science itself is divided into highly organised and specialised forces, with limited and specialised objectives. The problems we need to recognise and solve lie *between* these orthodox targets.

Thus everyone thinks, for example, of applying the science of hydrodynamics to hydraulic engines in a works; but few think of applying it to the analogous problems of the flow of a sliver of cotton through a doubling machine, or the flow of supplies of raw steel into a rolling mill. Operational research specialises in no particular science, but in the scientific method; it concentrates on targets which lie unnoticed among the official objectives of scientific application. We try to isolate and to destroy modern equivalents of the ancient contention that the world is flat.

(c) The Science of Decision

This is less the age of science of which we are inclined to boast, as an age of the awe of science. Thus it is not altogether surprising that, amidst so much scientific achievement, so much that is superstitious should survive. Neither is it surprising that these confusions should occur not in the field of creations but of decisions. It is in these circumstances that we sometimes call operational research the science of decision. There is, I hasten to add, no intention to arrogate the functions of management: only to make available to management decisions about those matters that are (in the language of logicians) "decidable".

Provided that claim is not misunderstood, I think no manager will object to the scientific taking of decisions. We are all conscious of the thousands of immeasurable variables that affect an industrial decision of any importance, and of the precarious assumptions that we have to make in taking it. In the morass of probabilities "Holy is lucidity And the mind that dare explain".

Thus I hope not to arouse adverse reactions on the lines that management is an art, a matter of human relations, and something which cannot gain from scientific help. Whatever I may say later, this Paper does not envisage an industry bereft of those prime qualities of management: leadership and understanding. There are always people ready to reduce an argument to absurdity, and then to complain because it is absurd. Those who would deny to management the services of science on the ground that science cannot *replace* management belong to this group.

From these introductory thoughts, I pass to a discussion of the industrial applications of operational research, noting some of its main techniques (the names of which appear in heavy type), and introducing some personal lines of thought to which their practice has led. Some rather arbitrary divisions will be introduced to aid the exposition. Firstly, there are some basic notions in this class of work which in my opinion underlie most applications. In Part I, I shall try to bring out what these notions are: illustrations must be specific, and none of those used should be taken as indispensable. This Part is intended to display a fundamental outlook: the orientation of the operational research scientist. Secondly, in Part II we shall review the major techniques so far available in operational research; these are the weapons in the operational research armoury that can be brought into use if the situation so requires.

Without wishing to pontificate about a classification which is mainly one of convenience, I do feel that almost any piece of operational research is likely to exhibit the characteristics of Part I, and within this general context to develop a variant of one (or more) of the major techniques listed in Part II. In the final Part, III, I shall give reasons for considering the science of cybernetics as inexorably involved in the future of industrial operational research. Another long Paper would be required adequately to indicate the scope for industrial cybernetics: Part III does no more than indicate why I believe that cybernetics is part of the scope for industrial operational research.

PART I
BASIC NOTIONS

I — COMMENSURABLE FACT

(a) Special Measurements

The first requirement of management must always be to develop an understanding of what is happening.

and to keep this understanding up-to-date. To do this, it requires a yardstick, a unit of measure. In a steelworks, tonnage is a favourite unit. The weekly output in tons goes down : management is told that smaller sizes than usual are being rolled. The unit of length may be used, and this figure drops : management is told that the sizes being rolled are bigger than usual. The unit of cost may be used, and this rises : management is told that there were metallurgical reasons for diverging from standard processes. And so on. Are these explanations reasons ? Are they excuses ? To what extent do the explanations in fact account for the discrepancies which the management is trying to evaluate ?

The first need in describing and understanding industrial processes is for a unit of measurement which takes into account the many casual variables which may affect results, and removes them from the field of argument. Ultimately, the unit of profit will do this; but this unit will also take into account variables outside the control of a specific area of management — such as foreign confidence in the pound sterling, which might affect an industry's price policy.

To measure the effectiveness of the production process, we need a yardstick of the productivity of the plant itself. But this measuring device must be sensitive to *fortuitous* changes in the products. In other words, regardless of what is being produced, it must measure the effectiveness of production. If this is to be done, we shall have to abandon traditional units of measurement, and resort to a *pure number*.

To be factual : if we make a ratio of what *could* be done with what *is* done, we shall have an index of productivity or effectiveness which is independent of all those factors we wish to exclude from our judgments. This is one example of commensurable fact.

I have described elsewhere in some detail (1) how this may be done ; the point to be made now is simply that it is both desirable and possible to create measures of what is happening which are pure measures, and impervious to special pleading. But it also leads us to consider the fundamental principles of such a technique ; these are important because they are common to most operational research.

(b) The Isomorphic Model

It is in fact the routine procedure in operational research to construct a model of the situation that is being examined. I use the word "model" as a piece of scientific jargon. The operational research model is not necessarily a mechanical one (although it may be) ; very often it is mathematical. The essential feature of the model is that it should be isomorphic with the situation it seeks to describe. I introduce this technical term, because there is no effective synonym : "isomorphic" means "having the same shape or form". The model is really a hypothesis about the way a situation operates ; the model becomes more and more isomorphic with the situation as more and more real-life characteristics are built into it. In the limit, the isomorphic model will behave exactly as the real-life situation behaves ; it can then be used to predict how real life will react to a set of hypothetical conditions.

The advantages of a model are basically two. Firstly, it is possible to predict what will happen in given conditions without putting these conditions into effect in real life — and possibly going bankrupt. Secondly, the model ought to be much more easy to manipulate than the situation it describes : it is usually possible to obtain a prediction from the model in a day which would take perhaps a year to evaluate on the shop floor.

Now all this is, you may feel, no more than a method of research widely used by technologists, who try out their ideas on a laboratory scale. In principle, this is precisely the case. However, whereas it may be possible to try out a large scale industrial pharmaceutical process on a small laboratory trial using retorts and glass tubing, it is by no means easy to think of an appropriate model to simulate such a characteristic feature of industrial life as the breakdown of plant. It is in the creation of models for situations which are not amenable to normal scientific description that operational research specialises.

The basic tool is that of *analogy*. We shall try to recognise in the situation we seek to describe the operation of some natural "law" already familiar to science in some other field. And it is likely that the expression of this "law" (which is to say the chief characteristics of its working) will be known to science in mathematical terms. A mathematical formulation is then available as the basis for the model we wish to construct. The final task is to adjust the model towards isomorphism with the target situation.

It has seemed right to make the specialised meaning of this word "model" as clear as possible, because it will occur continually in this Paper. For the moment, however, let us consider a model of the simplest possible form. Here is one set out to give the productivity, as a pure number, of several coils of steel strip which are being made thinner by their passage through a cold-rolling mill.

The total weight of these coils in lb. (w) is divided by the density (d) of steel and the cross-sectional area (b $\times$ g) of the strip. Having thus obtained the length of the material in feet, we shall divide it by the speed of the machine (v) : this is the time it will take to pass the batch of coils through the machine once. This result will be multiplied by the number of passes (p) through the machine required to do the job. If there is a certain handling time (h) associated with each pass, this time duly multiplied by the number of passes (p) and the number of coils (c) must be added. Thus we reach as a model of the objective time it will take to do a given job the simple formula :

$$t_o = p \left\{ \frac{w}{12\ dbgv} \right\} + chp$$

This model will be isomorphic with the shop floor situation insofar as the factors which comprise it can be measured with accuracy. Some of the factors, like density, are physical constants ; others, like the number of passes, may well require statistical research for their proper estimation. The productivity model

is now completed, according to the idea previously advanced that the effectiveness of the process can be measured as a ratio of the calculated to the actual time consumed. This gives:

$$P = \frac{100_c}{t_a}$$

where the actual time in the denominator has been obtained from an accurate shop floor record.

I again emphasise that this model is no more than a straightforward formula, and not one of the class of models developed by analogy with another science. But it does offer a concrete example of a model in which the effects (in this case on productivity) of varying certain components of the situation can be evaluated, and it does give point to the general case for measures of fact which are independent of the job-to-job variables involved.

(c) The Notion of Probability

Having dealt with some of the key ideas behind an operational research measure, I must turn to a second fundamental notion. This is the idea of *variability*. Suppose that we do obtain a real-life measure of some kind, and that we get the answer 48. Now this is something very definite: it looks clear and distinct, it sounds crisp and scientific. It is nothing of the kind. Such a number normally has two kinds of uncertainty attaching to it. Firstly, there is probably error in its calculation: the sort of thing we are really entitled to say is that the answer is plus or minus (say) 2. Secondly, this is either a solitary measure, or the average of a group of such measures. In either case, our answer 48 is probably (quite apart from the *error* involved) simply one of a number of estimates we *might* have obtained, had we taken a few more instances, or done our measuring on another day.

You will see how the comforting solidity of our "48" collapses under scrutiny. The real truth is something we never apprehend: it is always lurking elusively behind the measures we are able to take. Looked at like this, our answers are no longer seen as sharp points on a scale, but as areas, perhaps of considerable size, in which the point *probably* lies. We could shade in the area like a contour map: the shading would be light around the edges, where the truth is least likely to lie, and darkest somewhere near the centre, where the truth is most likely to be found. In short, any figures we use in calculations are labelled with a certain degree of probability — and we would be wise to find out what that is.

Picture one of these zones of probability like a contour map, and consider how it might be described. The scale on which we are trying to read our answer runs straight through it; let us treat this as we would treat a contour map, and produce a "profile" of the topography. The result would look something like Fig. 1.

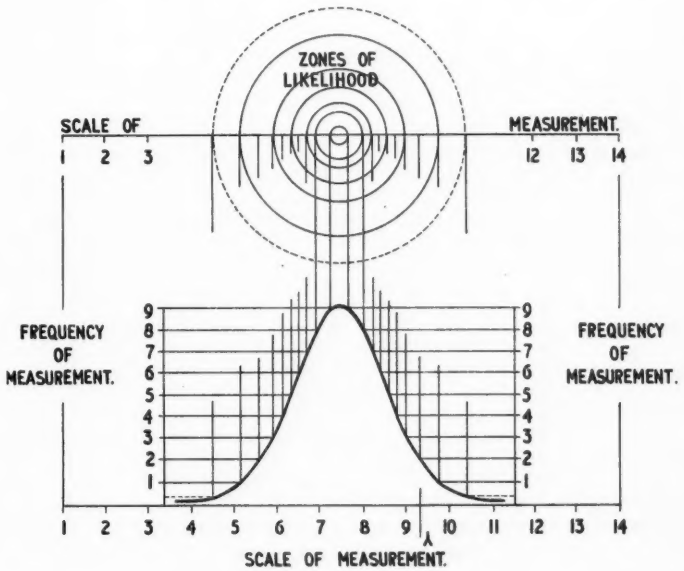
ZONES OF LIKELIHOOD
SCALE OF
MEASUREMENT.
1 2 3
12 13 14
FREQUENCY OF MEASUREMENT.
FREQUENCY OF MEASUREMENT.
9 8 7 6 5 4 3 2 1
1 2 3 4 5 6 7 8 9 10 11 12 13 14
SCALE OF MEASUREMENT.

Fig. 1. The contour map of probability.

The "contour lines" represent degrees of probability. The most probable point, at the centre of the concentric contours, is "the top of the hill" — that is, the *maximum likelihood*. Alternatively, the circles can be thought of as a target board; the practised player will tend to hit the centre and nearby most of the time; "outers" will occur with low frequency. A diagram such as that drawn is called a *probability distribution*.

"Probability is the very guide of life"; so remarked Cicero, who was not acquainted with modern industry. The fact is, however, that probability is certainly our best guide to the description and control of industrial processes. Thus the theory of probability has become the calculus of operational research.

Here we move on to difficult but firm ground. Fortunately, it is not necessary for us to draw little contour maps of all our situations. Mathematical statisticians have developed a vast corpus of theoretical knowledge about probability distributions on which we can draw. There are many classes of "hill" of the kind drawn above, many of which are not even symmetrical, and we are able to express these curves mathematically. A moment's reflection will show that the total area lying under one of these curves represents 100% probability: that is, our answer must lie somewhere within that area (bearing in mind that the ends of the curve *never* quite reach the base line). Thus if we want a measure of the precise probability of exceeding any particular value on the scale, all we are asking for is the proportion of the total area under the probability curve which occurs to the right of a vertical line drawn at that point on the scale. Since we know the mathematical expression for the curve, we can obtain a precise measure of probability by integrating between the limits of the value in which we are interested, and infinity.

How does this measure look, and how do we handle it in practice? Suppose that it would be useful to know the probability of finding a steel bar more than 20 ft. long out of a batch whose average length is 19 ft. Plotting the frequency distribution of the lengths over a sample of bars would yield a probability curve; this would be likely to have the shape of the one just drawn. That is, it would be of Gaussian form, which is often called the *normal curve*. The measurement required is the *proportion* of the area in the right-hand "tail", when the tail is cut off at 20 ft. This proportion is given mathematically as:

$$\Phi(x) = \frac{1}{\sqrt{2\pi}\,\sigma} \int_{20}^{\infty} e^{-(x-\mu)^2/2\sigma^2} \, dx$$

I mention this for people who have the vague idea that a normal curve is something imprecise that looks like a cocked hat. Obviously it is, on the contrary, something scientific and clearly defined. Secondly, for people who are convinced that statisticians always make things as difficult as they can, I mention that nobody need try to evaluate this expression: a competent textbook provides a table that gives the answer to anyone who can calculate the standard deviation of his list of bar lengths. Doing that is a matter of simple arithmetic. For the price of finding this out, a manager or technician can discover the risk of passing a bar more than 20 ft. long, or for that matter one of less than 18 ft. This kind of information can save money. Unnecessary inspection may be eliminated; the setting of a saw may be adjusted to cut out waste; the need for more accurate shearing may be uncovered.

With this simple but thought-provoking example of the use of probability theory, I end my list of the basic ideas behind operational research methods. We look at situations through the analogy of an isomorphic model; we seek to make our measures of fact commensurable within this frame of reference; and we handle our facts, which we know to be both sacred and imprecise, in terms of their probability.

II—THE CONTROL AND USE OF MEASURES

(a) Statistical Control

If we can create a satisfactory measure of something that appears to be insusceptible to measurement, we have taken a first and vital step. Colloquially, when a difficult situation has been mastered, we speak of "having its measure": there is wisdom in that phrase. But too many studies of situations, and too many managerial exercises, treat the obtaining of a measure as the climax and conclusion of understanding. A sheet of good data does *not* confer on its possessor magic powers over the situation it defines. Measurements are important *because they breathe the life of quantity into an inert model.* People who act on their first reactions to these data must in reality be assuming some sort of model in the back of their minds: it is probably a very bad model indeed.

For example: suppose a manager to be confronted with a cost sheet that shows a deviation from standard (or average) cost of £5. Suppose that he is then galvanised into action, and begins an extensive post-mortem into this manufacturing loss. Why does he do this? He does it on the basis that "£5 is a lot of money". In fact, his model derives from his own personal economy; were £5 missing from his wallet he knows that his reaction would be intense. Another manager, however, might do nothing at all. Why not? Again, he may well be working on the wrong model. In the back of his mind something is saying: "this company has a turnover of £Xm per annum: what is another £5?". Neither of these approaches is right: the proper criterion of the seriousness of this particular loss is the *variability* of the cost natural to the process on which the loss was incurred.

Suppose that the average cost of this process is £100. If, due to the variability of the process, that average stands for a series of results varying apparently indiscriminately between £50 and £150, then it is virtually meaningless to talk about a discrepancy from average of £5. The first manager mentioned is not only wasting his time but the firm's money: his post-mortem might well cost the company £50 in time and lost production. On the other hand, the natural variation of the £100 average cost might well be from £97 to £103. In this case the reported discrepancy of £5 is clearly of vital importance; this

is not because it threatens the company with bankruptcy, but because it means that the process is in some sense out of control. The manager who ignores this signal may eventually lose a more serious sum of money. The moral of this parable may be that the use of averages is dangerous, which is true. But the real answer to the problem is to set these measurements in the right operational research model (rather than some unexpressed subjective model of the man who scans them) and thus to get control of the measures themselves. Once the model is under control, it will be possible to control the real-life situation.

Again, I am placing a simple interpretation on the term "model". Later on, we must interest ourselves in complex models of situations where the interaction of a number of causal networks has to be taken into account. But if that is the final model, we must first consider the sub-model for an individual measurement. This, as the parable indicated, is its natural variation. Now the variation itself has a "shape". It may be considered as a probability distribution. After all, if a quantity normally varies between 50 and 150 the probability of the occurrence of these extreme values is obviously likely to be low, whereas values around the 100 mark might be expected to occur with greater frequency. A statistician can readily determine the mathematical function which describes the variability of a particular measurement. For reasons explained in the last section, this means that he can quote with accuracy the probability attaching to any particular value. Let him now determine two points, one on either side of the average value, such that there is one chance in 20 that this value will fall still farther away from the average. These two values are known as *confidence limits*. That is to say, if a value is discovered which lies between these two points it can cheerfully be regarded as "belonging" to the population of values appropriate to this particular measurement. To regard a value which falls *outside* either of these points as "normal", is to accept odds of at least 20-to-1 against. This particular confidence level is arbitrary: but most people would be prepared to agree that odds like these are too long. If a value comes along at this level of risk, it is suspect. We have now got a useful model. In the light of this, that £5 discrepancy will appear (as it should) as insignificant in the one case, and as *significant* in the other. (That word "significant" is a technical term in statistics: it means that a value has occurred which cannot be attributed to chance variation.)

Many people will by now have realised that I am talking about something which is quite familiar to them: statistical quality control. I have had two reasons for not admitting this before now, and for developing the argument in the way I have. Firstly, it is often supposed that statistical control can in fact *only* be applied to assist in the maintenance of quality in a product. Many will have seen it applied, for instance, to a key dimension on a piece which is being machined; here the object is to keep the machine setting within acceptable bounds. But the methods can be applied to any measurement whatever. Secondly, because statistical quality control has been made easy to apply by the provision of special tables and charts, some people have come to accept it (or not) as "something that worked (or did not)". My object has been to expound, not the statistical theory which can be obtained from any elementary textbook, but the underlying and surely quite sensible principles behind the technique.

(b) Managerial Controls

To those who feel that it is condescending to devote so much time to this elementary explanation of something they might regard as well-known, I would like to put a question. Why, then, is this technique so little used? I would like now to make a plea for the extensive adoption of statistical control of this type, not so much on the shop floor as by management itself.

The comparatively recent creation of management as a subject of study has led to a much wider understanding of the class of measurement it is proper to collect, and of the importance of accuracy and speed in its presentation. All this has been very much worthwhile. But it has inevitably led to the inundation of management by a sea of figures. Data pour on to the manager's desk: figures relating to production, sales, costs, wages, research, absenteeism, accidents, and so on. The more the message about the vital importance of measurement is propagated, the higher the flood waters rise. The more senior the level of management, the more threatening does this spate become — despite a certain amount of consolidation of figures as they rise to the very top. (Please note that even this obviously desirable process of consolidation, since it normally works upon averages, is unfortunately constantly swallowing the all-important information about *variances*.)

Clearly there is a problem here. In the first place, management desperately requires some guide as to what it ought to look at, what is important, what is *significant*. And, in the second place, there are fortunately still managers who wish they could spare more time from their desks to look and to think. Would not the wholesale adoption of statistical controls provide an answer?

For my example under this heading, I will take that of the productivity indices mentioned in Section II. This is a factual example, and may answer a criticism which might be forming — that, since I feel so keenly about the quantity of information being thrust at management, I should not have advocated more!

Consider a department in which, over a period, some 20,000 machine-jobs have been carried out. The manager of this department obviously requires some measure of the effectiveness of the work. By the use of indices, as previously explained, this difficult assessment can be made relatively easy — by taking all the variables out of the judgment. Is the manager, however, to be faced with 20,000 productivity indices? The answer is "No".

First of all, we shall build another model. So far, we have a model which facilitates the study of productivity of individual jobs. The new model will be statistically more complex: it will indicate

the inter-relation of individual jobs, in terms of the common measure that we were at pains to invent. Whereas it would be fatal to attempt a grouping together of this vast amount of data in terms of some conventional breakdown of orders, it is a fascinating operational research job to create the *departmental* model of the general pattern of productivity disclosed by the measurement of indices. This new model will produce a fairly small number of productivity groups still perfectly amenable to statistical description in terms of probability. In practice, data of the extent and complexity described tend to fall into about 80 such groups.

Secondly, there is an important off-shoot of the scientific study of probability, known as sampling theory. This teaches how to choose a relatively small number of incidents, as representative of the totality. There is no need to go into the details here: I merely emphasise the mathematical validity of the practice, and point out that by the use of properly instituted sampling techniques all the required information can be obtained by studying perhaps a mere 10% of all the incidents concerned. Thus instead of evaluating indices for 20,000 jobs, only some 2,000 need be evaluated — spread over 80 groups.

At this point we invoke the notion of statistical control. The control statistician has to examine the final 80 results. The great majority of these will, naturally, be in control: that is, they will fall within the confidence limits calculated for the appropriate group. Usually we find that some five or six results are *significant*: these are the ones to report to the management.

It is important to realise how far we have now progressed from our original demand for measurement of some kind. Even in the simplified productivity model quoted previously, there were eight major variables. Thus the total information involved in this departmental study of which I am speaking would consist of 20,000 sets of results, to each of which would be appended a list of eight relevant factors. Surely it is fair to say that no living person could comprehend this mass of information, far less gain a genuine understanding of productive effectiveness from it. By the use of operational research techniques, and (to this point) only those of a most elementary kind, all this is *scientifically digested*. The manager finally receives all that he wants to know on a single sheet of paper; he has a statistical guarantee into the bargain, that anything which is not on this piece of paper is behaving within the normal limits of chance variation.

This example is a factual one, as I said, and it has proved perfectly practicable to install this control system as a regular, routine feature of a steelworks management.

(c) Controlled Estimation

The need for measurement of an unorthodox kind has been discussed, and an example has been given of a fairly sophisticated indexical technique. The handling of these measurements by methods derived from the theory of probability has been described in terms of establishing a system of description using the notions of maximum likelihood and measured risk. Such a system is flexible; it will not shatter irreparably into fragments in the face of the obstinacy of men, the vicissitudes of production, or the malice of mischance. The system, once set up, can be kept under permanent surveillance by the statistical control of its key elements, the behaviour of which is already understood in probabilistic terms. All this has been illustrated, up to the point of reporting to management, in terms of productivity indices. More positive use can, however, be made of this system: I will give brief descriptions of two such active applications, fuller details of which may be found elsewhere. The first refers to production control (1), and the second to cost accounting (2).

A production control department must produce production plans, and these must involve the creation of detailed machine load programmes. Many production control systems are ineffective because they do not face up to this fact. The reason usually given is that it is impossible, particularly in a jobbing shop, to estimate with any accuracy the time taken to do a given job on a given machine. If really reliable estimates of machine-job times were available for any job on any machine, and especially the class of jobs which have not been done before, then there would be no particular difficulty in constructing a workmanlike production plan. It would be constructed like a mosaic from the component machine-job times. If this plan, in addition to being reliable, were also resilient, then it would not be reduced to chaos as soon as the exigencies of production required its modification.

All these requirements can be met by the active use of a system set up by operational research methods such as that we have been discussing. To estimate a machine-job time, it is necessary first to evaluate the constructed time by exactly those methods described in Section II (b). The same model, and the same notional factors, that were used when the system of description was being built are used again. That is, we calculate an *objective time*. This result has been generated by a model which is known to be isomorphic with the shop floor situation, and the quantitative relationship between the two is measured by the productivity index. Consequently, if we now *weight* the calculated result by the current index appropriate to the job being considered, we shall reach a statistically guaranteed estimate of the actual time the job can be expected to take.

The details of this procedure cannot be mastered without further and lengthy explanation; but the principle is not difficult, although it is very important. It is widely recognised that there is a difference between theory and practice: this operational approach not only acknowledges this difference, but measures it continuously, and uses the statistically controlled measure (that is, the index) to improve the estimate. Engineers will recognise the principle of "feed-back" which is being applied, not mechanically, but statistically.

This method is *lively*: it is not a static moribund technique which begins well enough and is then left behind by the changing modes of behaviour which characterise the natural evolution of any works. The claim that it will apply to work which has never been done before is justified in the comprehensiveness of our operational definitions. Any occurrence has a statistical probability; this maxim can be applied with great effectiveness to the likelihood of a given job falling within the ambit of a given model. The resilience of a plan built from components evaluated by these methods is given in their statistical control. For the value taken as an estimate of machine-job time is that determined by the maximum likelihood of its probability function. When these times are built into a machine load, there is a "give-and-take" between successive estimates, also determined by that function. It is as if the bricks with which we are building were set in place, not with a brittle mortar, but with an elastic bond capable of considerable deformation. It is this property in the plan that I call resilience. In practice it provides that the plan can draw out of phase with the production it controls *without* disintegrating: by taking into account the range of an estimate's variability, the plan acquires what I shall again call the *lively* quality of adaptability.

This is perhaps the moment to comment on the function of *prediction*, which is a major application of operational research. Planning of any sort is one of those activities in which it would be very useful to know what is going to happen tomorrow, or next week, or next year. In applying itself to this problem, operational research normally uses a statistical technique (such as the one described, or the branch of analysis known as Time Series), in which probabilities are examined in their behaviour within an isomorphic model. Now it is woolly thinking of the worst kind to imagine that such a technique will enable management to look into the future; for such a purpose I would as soon use a crystal ball or the entrails of a sheep. If, then, we cannot in fact look into the future, in what sense is the word "prediction" used? Suppose that I put a kettle of cold water on a gas ring. I *predict* that in a few minutes' time steam will issue from the spout, and the water will be boiling. Suppose that I issue cheques to a total value greater than my bank balance. I *predict* that at some future date I shall receive a message from my bank manager. Each of these examples shows me making an intelligent anticipation of something that has not yet happened: and yet you will not say that I am gifted with second sight.

Prediction of an intelligent or scientific kind involves induction: the amassing of particular instances leading to a generalisation which we dignify with the title "law". The extent to which we are prepared to act upon the law depends upon the strength of the evidence which supports it: we are still in the realm of probability. That the kettle will boil is so likely that people often fail to realise that they are *not* dealing with a certainty, but with a very high probability. Why is the probability high? It is because the evidence is simple, clear-cut, extensive, and highly organised. Scientific predictions can be made *to the extent* that these characteristics are found. The main point of these remarks is this: everything depends upon our discovering the "law", which is the same thing as saying that we have to find a model. The law or the model is the framework within which we organise facts. People sometimes seem puzzled by the readiness with which an operational research man will predict (say) rates of stoppage on production plant, compared with his diffidence in predicting (say) the company's final profit. I would ask them please to remember these basic principles.

The point about cost accounting can now be made quite simply. When an order is received, we want to *predict* its cost. The modern accountant is well aware of this: the practice known as "historical costing" is quite out-moded, at least in up-to-date firms. Yet the accountant, like the rest of us, is debarred from looking into the future. Therefore, he has much to gain by invoking operational research. He could fairly expect operational research so to organise his data about raw materials, about overheads, and most of all about output rates, as to introduce *liveliness* into the costing system in the same sense as that notion was earlier applied to production control.

In fact, this idea has been tried and proved on quite a large scale insofar as output rates are concerned. These can be generated by operational research techniques in just the same way (moreover, by the self-same system) as the planning predictions were made. Figures supplied for costing purposes on these bases have the following advantages:- they can be generated very quickly on an electronic computer or other high speed calculating machine; they do not require constant overhaul by the works management, because they can be controlled by the routine statistical methods already installed; significant departures from the standard or average level are automatically signalled by the statistical control; above all, they are *predictions* in the sense described above.

III — SOME IMPORTANT COMMENTS

(a) Complexity versus Simplicity

Before leaving this discussion of the basic notions behind operational research, there are three comments on what has already been said that I wish to make with some force. The first concerns the complexity of this approach. Fairly simple language has been used, but operational research has probably emerged as rather a complex subject. Is it too complex? What about the virtues of simplicity? It is vital that we face up to these questions, and to their answers.

Before anything else can be done, a situation must be described both operationally and dynamically. Since the situations in which we are interested are of great complexity, because they involve machines and men and advanced industrial practice, the operational research model must itself be complex. There simply is no short cut at this point. Unless the model

parallels the situation in complexity, it cannot attain to isomorphism, and cannot predict. This is not to say that the model must needs be as untidy as the situation ; indeed, the virtue of these methods considered as descriptive techniques is that they reduce incommensurable measures to pure numbers ; and they reduce variability, risk, chance and misfortune to a system of probabilities of great neatness. But the complexity of the situation is reflected in the difficulty of the algebra : it is still there.

From this basis a result is obtained : a solution, a comparison, a prediction. At this point, the point of presentation of results, simplicity becomes the real aim. Everyone knows that results must be expressed as briefly, clearly, and simply as possible. And yet managers are constantly irked, often to the point of exasperation, by a failure to observe this rule. It may help if I suggest for your consideration two reasons why I think these troubles arise. One is a severe practical difficulty, and the other is a psychological one. Sufficiently wise management can deal with both.

Firstly, the operational research man is anxious that his conclusions should not be misunderstood. He understands the complications of the subject he has been investigating, and inevitably feels that the concise answer he would like to submit is an oversimplification. He is therefore tempted to bring in reservations and qualifications, to elaborate his point, and to expatiate upon the methods he has used. There are two answers to his problem, I suggest, and the initiative in both cases lies with management. If the operational research analyst could know that the people he tries to help already understand the nature of his subject, he would not feel so diffident. It rests with management, when they introduce work of this kind, to see that the staff is fully informed. This surely means that the scientist must be given the opportunity to explain himself and his techniques: something like a deliberate educational policy will have to be launched. But the operational research man needs educating too. He has probably been brought up to believe that his job as a scientist is to determine the facts, and to leave management to make the decisions. He feels this responsibility keenly, and thus becomes too scrupulous.

This brings me to what I consider a vital difference between the scientist who is engaged on pure research and the one who does operational research. In the second case, because his methods are management tools, the man himself partakes of the responsibility of management. If management wishes to decide between two courses of action, operational research should present the facts on which it can base that choice. But in determining what are the facts to put before the management, the analyst has to take a number of less important decisions himself. If he does not, he is asking the management to decide not only the major issue on which it asked his advice, but on a hierarchy of subsidiary decisions as well. This is where genuine scientific scruples lead to utter confusion. The operational research man wants to say : "if Mr. Jones was telling me the truth . . .". Management must teach him to accept responsibility for deciding whether Mr. Jones was telling him the truth ; the manager is not in a position to say.

Secondly, I think it is only human to recognise that the scientist who is trying to make the simple report that he knows is required, has his own personal and professional pride. When he has worked hard and wrestled long with an intractable problem, he does not want to be told that his "guess" has been noted along with those of others who he knows have scarcely considered the problem. In short, like everyone else, he does not want to be underrated ; and this leads him almost unconsciously to give his reports that air of importance which the management will regard as incomprehensibility. If managers would understand this human shortcoming and treat it sympathetically, much of this difficulty would disappear. They can, after all, kill two birds with one stone ; for the suggestions offered in the last two paragraphs will answer this second, psychological, question as well.

Finally, there is a class of operational research which does not result so much in a report to management, as in the installation of an operational solution — a method to be adopted in the works, or a system of control to be installed. Here again simplicity is the keynote. It is rightly demanded that procedures that have to be handled by work-people or clerks should be within their capacity to operate. Thus operational research should give the closest attention to translating its original (and justifiably) complex treatment into a workable form. To do this, it may be necessary to install permanent operational research controls at the centre to ensure the continued validity of the simplified translation which is being used in the works. A central control of this kind is often to be found, and should be regarded as the real solution to the paradox of complexity versus simplicity. In effect, this practice divides the problem of handling a difficult situation into two parts : the complexities are removed from the problem and handled at the centre, and large-scale applications can then safely be dealt with by more junior staff in the works in terms of simplicities.

(b) The Question of Productivity

The second of these comments concerns not operational research itself, but one of the applications discussed earlier in this Paper. Mention was made of productivity measurement of an advanced kind, and the various uses to which such measurements have been put were described. Since operational research does not engage in a study of this kind without learning a great deal about the mechanisms involved, it would be remiss if I were not to offer some of the conclusions about the question of productivity to which I have come. It is widely claimed that our national difficulties are only to be overcome by higher productivity. This means that productivity is not at this moment as high as it could be ; that some of it is "missing". Where is it ?

There is a glib answer to this question, which says that "no one works hard these days". Therefore, it is argued, the missing productivity is up the sleeve

of the operative. This is no more than a half-truth. If *within the accepted structure of a man-machine process*, all the labour factors in the operational research model are set at conditions of 100% effort (that is, at a level of "sweated labour"), my experience is that an overall increase in productivity of perhaps 2% or 3% is indicated by the model. There is great scope for increasing productivity, and yet *within the system that is being worked*, intense human effort cannot make much impression.

We cannot be satisfied with this: the models must also be made to say where the missing productivity has gone. This they can do. Consider a large plant as an organic whole — which is what it ought to be. Its smooth flow of material, the fitting together of the separate parts of production, the need neither to overload the digestion of any machine nor to starve it, the purification of the blood-stream that supplies the vital services, and the health of the nerves that carry the communications of control: all these *organic* requirements are the criteria of efficiency.

From our models we learn that for every few per cent. of increased productivity that is obtainable by sheer labour effort, something between 20% and 50% is available in terms of better "body tone", of truly organic behaviour. This could be achieved, according to our research, in terms of (and I am going through the physical analogies just drawn) smoother flow, better planning, better machine loading, better servicing, and better communication and control. These aims are doubtless already objects of management everywhere; but I am not speaking of a mere improvement in present conditions. To achieve real liveliness, or should I say this time *life-like-ness*, a step is required from our present concept of an "integrated works" to the concept of an "organic works". The features of a factory wherein this organic wholeness lies, which I have listed, need rethinking, not just improving, on a scale not yet imagined. This is not an argument from the inevitability of technical change; it is an argument from the operational research model. Here alone we have the opportunity to understand the interactions of the parts within the organic whole.

Imagine processes A, B and C. Process B tends to be a bottle-neck, although a work study investigation has revealed that there is plenty of spare B capacity. A large incentive bonus is therefore applied to process B. Production and wages rise: but there is still a bottle-neck. And yet there is reason to think that labour effort is still well below standard. What has happened? If we plot the probability distribution of real effort (say the productivity index), we are likely to find statistical truncation; that is, instead of the usual "tail" on the right, there is a sudden precipice. This means that *unnatural* limits are being set to effort in process B. Either the labour-force has carried its bonus earnings to a point where they are as high as it thinks management will permit, or the flow from process A and into process C is too erratic or unplanned to permit a high rate of B productivity. This is what I meant by talking about "the accepted man-machine structure". Although labour is not working very hard, it is effectively prevented from working harder by either an economic or a manufacturing constraint. If operational research is asked to study the man-machine system A - B - C as an organic whole an answer may be found; possible methods will be discussed in Part II.

Finally, there is a point of great economic importance. Changes in the whole nature of the organic functions mentioned would presumably involve some capital expenditure. But this is not expenditure on a large scale: the factory to all appearances would remain unchanged. By its "animation" we reach towards the full utilisation of existing resources. At a time when industry is preoccupied with large scale expansion, this utilisation of existing resources sounds a dull theme. The phrase itself is colourless jargon. I will use a more ancient and more telling admonition. "Milk the cow that is at hand", said Theocritus, "why pursue the one that flies?".

(c) Integration with Management

This point can be made briefly: it follows as a corollary of (a) and (b). Operational research is in fact *operational*; its results should be built into management technique. The full rationale of this thesis appears in Part III.

The examples that were given earlier were deliberately chosen to demonstrate how the operational research approach can be integrated with management functions, can be made implicit in the system whereby the factory runs. Once methods of this kind have been installed, a manager should have the surety that when he looks at data, he is not being set a series of traps in which he will become ensnared unless all his experience and judgment are brought to bear with constant vigilance. He should be able to feel that operational research has ground a scientific outlook into the lenses of the glasses through which he views the operations under his control.

In the succeeding Parts of this Paper I shall deal with more specific tools than those basic notions I have so far described. All these presuppose the ability to measure, to describe, to control, and to predict, by the application of probability theory to operational research models. For simplicity, these more advanced methods will be discussed as if they referred to investigations which, once completed, would be forgotten. This is, however, a deliberate concession to space. Please bear in mind how each of these techniques would in practice be an integral part of managerial activity; operational research must never hurl advice at the players from the touch-line.

PART II
PROBLEMS AND TECHNIQUES

IV — STOCKS AND FLOW

(a) What is a Stock?

Consider a number of people arriving at a football match. There is a turnstile. Individuals arrive, their money ready, at random times. Someone is

issuing tickets: he takes a random time to deal with each client, which depends on several factors — the giving of change, for example. Sooner or later we find that the entrance has become congested; a number of people has accumulated in front of the turnstile, and we call this line of people a queue.

Suppose that the people arriving at this gate represent orders arriving to undergo a works process. All these orders, let us say, are late for delivery, or have top priority. The object will be to process them as quickly as possible, to hurry them past the gate, just as the issuer of tickets is trying to pass his clients into the football ground. But the process time is variable, depending on several factors. So in a works also there may be congestion; again a number of orders may accumulate in front of the "gate"; again we may call this a queue.

Once some facts about this kind of process have been collected, they can be put into statistical form. The *arrival times* are distributed in a statistical sense: that is, it is possible to work out their probability function. The *service times*, too, have their characteristic form. Now the operation of this gate, the process of passing an arrival through the service, consists in collecting an arrival time and a service time as a pair. There is no connection between the two: the fact that someone arrives two minutes after his predecessor has nothing to do with the time it will take to service him with a ticket. And yet the result of such arbitrary pairing characterises the operation of the gate; a large number of such random pairs will define the queuing problem.

Since the two statistical probability functions are known, it becomes possible to work out the risk run of forming queues of various lengths mathematically. It is also possible to investigate how a queue can be eliminated by increasing the number of service points (for example, of cranes or soaking pits dealing with hot steel ingots), and how much waste of service capacity (for instance, of cranepower or heat) will be incurred thereby. The body of work that has been done on this type of situation is now quite large; it is corporately known as the **Theory of Queues.**

Queue Theory, in short, enables operational research to determine the characteristic of a situation where a queue tends to form in terms of probability. It does not matter whether the queue consists of people wating to go into a football match, patients waiting to consult a specialist in a hospital, aeroplanes "stacked" over an airfield, or orders waiting for processing in a works. A clear and easily followed account of the use of simple queue theory is given by Ramm (4) who applied it to find out the number of staff required to give good service in a retail shop. Ramm based his method on mathematical queuing relations developed by Kendall, whose classic Paper (5) may be consulted by mathematicians.

When people or things collect in front of a gate or process like this, we normally wish they would not. The word "queue" suggests some form of nuisance. There are phenomena, however, which are exactly of this kind, of which we normally approve. When we approve, we do not speak of a "queue" but of a "stock". It may be unusual to view stocks in this light, but it pays. A stock is defined as a buffer between two probabilistic processes; it is there to absorb the risk that the first process will not be ready to disgorge an item to the second process at the right moment. This is true for many pairs of processes: machine 1 to machine 2, department A to department B, raw material supplier to the works, the works to the customer, and so on. Many kinds of stock are differentiated for management purposes, and sometimes their different names are useful: raw material, inventory, work-in-progress, "agreed", minimum, maximum, and so on. But the different contexts in which these kinds of stock are mentioned ought not to obscure the basic fact that a stock is a desirable queue, and that its purpose is fundamentally always the same: it is a safeguard against a measurable risk.

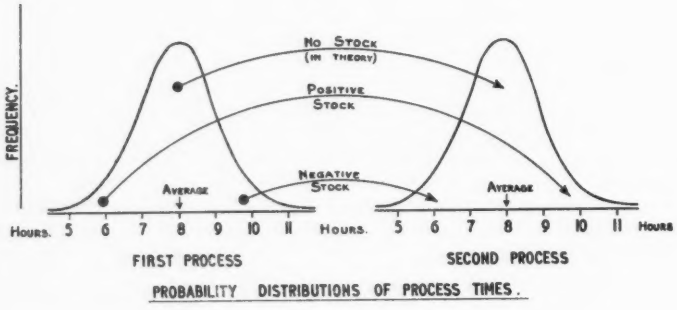

Fig. 2. The nature of a stock.

This is illustrated in Fig. 2. If there are two processes, each with a cycle time averaging eight hours, how much stock is required between them, if the second process is never to be stopped for want of work ? Suppose we do not deliberately set up a stock at all. When the two processes happen to take exactly the same time over the jobs they are doing, the first can pass on a job to the second at exactly the right moment ; no stock is created, and no time is lost. But if the first machine happens to have a good run, and takes a short time, while the second machine has a bad piece, and takes a long time, a " stock " piece will lie between the two machines until the second machine is ready. This is a positive stock, or a queue. If the positions are reversed, the second machine will be waiting for work ; not only will there *not* be a stock piece in front of the second machine, but there *ought to be one*. I call this a " negative stock " : it is a sort of production vacuum. In deliberately creating stocks as an insurance, we are trying to fill this vacuum. The size of stock required to do so can be calculated to a nicety in terms of the probabilities attaching to the creation of " negative stock ". From the diagram you will see that this risk depends upon the length and "fatness" of the two " tails " facing each other. It is just this tail length and fatness that are measured in the probability functions which define the two processes. You can imagine that the mathematical statistician is capable of calculating the risk of " pairing " across the tails.

By this approach operational research is enabled to recommend proper stock levels. We cannot *guarantee* that a given level of stock will always be adequate. The probability tails never quite reach the zero line : there is always some risk that any stock will not be adequate, then, unless that stock is infinitely large. But we can specify the proper level of stock for a given level of risk. That risk can be made negligible. But it is more important to realise that by accepting a larger risk, stocks can be drastically reduced. Industry has been so concerned with high productivity (and rightly so) that stocks have often been allowed to rise to the point where the protection they confer on the " second process " could be classed as over-insuring it. This is not surprising, when the fixing of stock levels is considered. I suggest that a stock level is often the resultant of three forces : the production man's personality (he wants security), the financial man's personality (he want capital), and the availability of space. Again, there is often a high correlation between stock levels and some standard time period like a week or a month. But what is the sense in holding " a week's stock " ? If there is a time cycle of a week built into the supply situation, there may be some sense. But to say simply that " a week's stock is about right " has nothing to do with the science of the situation which, as we have seen, depends upon the probability functions of supply and demand.

(b) Organic Flow

The queuing theory we have been considering is relevant to queues of another kind. In many industries, a number of machines (looms, for example) come under the care of a single operative. If anything goes wrong, the machines themselves may begin to form a queue for attention. This is the problem of machine interference, which the theory of queues can also tackle. But considerations of this kind lead into rather a different subject, because they concern *renewals*. That is to say, there are many industrial entities which, like their human invigilators, are born to die. The " birth-and-death " process, like the queuing process, depends upon an element of randomness. But whereas in queue theory we specially noted that there is no connection between successive time intervals (because they come from independent populations of times), in this new kind of process there is every connection.

A tool or a machine that is wearing out passes from one state to another, until a final irreducible state is reached : " death ", the moment for renewal. Each succeeding date depends upon two things : firstly, the preceding state ; secondly, what happens next, which (since we cannot specify it) we may call a random influence. Something which proceeds in this way from one state to the next can be described by what is called mathematically a Markov Chain. This is a descriptive name for a series of states in which each depends on its forerunner alone, and its formulation creates yet another kind of probability model. With the aid of this model, or any of its modifications, a great deal can be said about processes which are born to die. Wide use has been made of this approach in theoretical physics ; but what about industry ? With this tool it is possible to calculate the increasing risk of breakdown with age. At a specified risk of breakdown, then, which would vary with the importance of the plant concerned, it is possible to withdraw the plant from service at an opportune moment. The production engineer who has ever interested himself in *preventive maintenance* will surely be interested in an operational research colleague who is armed with this particular weapon.

Now a Markov Chain is a particular example of a class of process known as a Random Walk — simply because each state succeeds another without predetermination. In this general case, we can drop the Markov restriction that only the immediate previous state affects the next, and calculate the effects of several intruding factors. And so the list multiplies. There is a " contagion " process, in which one person passes something on to another : this something could be the measles (the theory has already been applied to the study of epidemics) but equally it could be information. Information Theory itself is another example of the class of work we are considering, which may be familiar to engineers for other reasons. A great deal could be done to study industrial organisation scientifically by such operational research methods ; the prospect has been discussed in another Paper (6).

Queuing, machine interference, renewal, birth-and-death, Markov Chains as types of random walk, contagion, information theory : all these models have been briefly mentioned. Why ? Because they have a common characteristic : randomness. I am seeking

to convey with all the emphasis I can that operational research is concerned with a real world, a world in which chance and mischance play leading roles. If operational research has one special claim it is that it conducts industrial science out of the if-this then-that laboratory, and into the real world of perhaps and maybe. Behaviour which is characterised by a random element, and I have just cited eight different kinds, is the commonest form of industrial behaviour. Whatever its kind, we refer to it as a **Stochastic Process.**

The word "stochastic" comes from a Greek word meaning "to aim". Our industrial behaviour, once we leave the production line itself (and all too often while we are still on it), has only a general aim, which it usually attains — more or less, sooner or later. The methods of getting there are the stochastic processes. These are the major factors that were listed in Part I: planning, communicating, maintaining, and so on; above all, stocks and flow. These are the *organic* features of the factory. If the factory does not really operate as an organic whole, it is because we do not properly understand the stochastic behaviour of these vital features. Stochastic models in the hands of operational research can provide this understanding.

I have said really very little about treating this organic flow by which I set such store: the subject is too big. Those interested in stochastic models can read about them in Bartlett's book (7). The main point I want to convey is that there is this way of looking at the flow of vital materials and forces through the industrial body. Nothing will flow through a dismembered corpse; we need a living being. The outlook can be cultivated independently of operational research techniques.

(c) Facts and their Simulation

The whole of this section has been concerned with Stochastic Processes. This term, while it does apply to the physical situations described, also applies to the mathematical models of the situations. Strictly, it takes a mathematician to formulate and to operate these models. Let no one assume, however, that to hire a mathematician is to hire an operational research analyst, nor that operational research is a form of mathematics.

Mathematics is the common speech of science, and the main language of probability theory. It is the most satisfactory way of expressing complicated quantitative ideas. But, while it is great fun to construct mathematical models like castles, glorious, forbidding, and crowned with algebraic flourishes like pinnacles, they are useless if built in the air. The operational research mathematician is someone who understands this; someone who knows that his model is only as useful as his *works data* tells him what he needs to know. Notice that I do not say that the data must be "absolutely accurate": the statistician in us ought to regard such a demand as naïve, if not meaningless. We must understand our data and their limitations: this is vital.

How is the information to be collected? Preferably, by institutional means. Shop-floor records, with all their faults, are usually best. These records may be inaccurate, illegible, missing, "adjusted", or blatantly mendacious; but remember that they have been near to truth. They were around when the job was done. A crisp, clean, perfectly printed set of information can so easily turn out to be a beautiful liar: do not be gullible. This nicely packaged set was probably synthesized *from* the shop-floor record, quite probably by a clerk more anxious that all the figures should fit together than that the facts should emerge.

Another valuable source of information is the work study. The active help of this kind of technique is invaluable to operational research. It is usually possible to understand the limitations of these data, because an astute work study engineer knows very well how accurately he can establish the facts of a given situation. The two real difficulties are, I think, that it is hard to ensure that the study is truly *operational* (because by measuring facts you alter them, as every physicist knows), and that you cannot invoke a basically observational method if there is nothing to observe (much operational research concerns policy, planning or some other hypothetical situation). In a practical job which means something to a production engineer, however, work study and operational research have a great deal to offer each other. Operational research ought to gain sheer information; work study can be put in the wider, organic picture of interacting probabilities. Apart from anything else, it can be guided to attend to activities where control is being lost, inefficiency is locked into the accepted pattern, or risks combat with prudence.

Assuming, at any rate, that a suitable stochastic model is available, and that the relevant data to generate through this model have somehow been assembled, there are still two major hurdles to be cleared. Firstly, it can happen that the stochastic process is so complicated that there is no mathematical method of solving it. Remember that the queuing process as we discussed it related to the convolution of two probability functions. It is unfortunately possible to construct more complicated models, which are clearly isomorphic with reality, but which defeat pure mathematics in its present stage. Secondly, whether the mathematics can be entirely organised or not, to *evaluate* the model for a given set of facts may present serious problems in the very mechanics of calculation. If either or both these hurdles cannot be jumped within the ability of mathematical processes, another operational research technique may be used.

This technique is to take the established model, and to generate known information through it *experimentally*. Instead of "solving" the model, we operate it and see what happens. In doing this, the problem is simulated by operational research, much as a game of Bridge is simulated when all the hands are spread out on the table and a "model" game is played with all the risks and chances taken into account. To do this, it is necessary to work from the actual distribution of data represented by the

model's probability functions. We should select a random value from each distribution (which is exactly what happens in real life) and build up a synthetic works record. Looking at Fig. 2, we see how those all-important pairs could be selected, by choosing values alternately from under the two curves ; if the selection of a value under each curve is done genuinely at random, the probable frequency of the occurrence of each value will imitate reality. That is, there will be, as there should be, a greater proportion of values near the "maximum likelihood", than in the "tails" of the distribution.

This is a truly scientific procedure, because the *model is in action*, although the real-life element of chance is now being introduced by a deliberate randomising technique, instead of through mathematical statistics. It is because of the randomising that this type of solution has become known by the facetious name of the **Monte Carlo Method.** It is a most useful technique, which can be put into use either by hand, or (in really vast problems) on an electronic computer.

One of the advantages of a Monte Carlo solution is that, making a virtue of necessity, its technique can be understood by people who do not understand advanced algebra. If they can see the model working, and producing its answers, they may obtain a real insight into the organic flow the model depicts ; this insight is otherwise a direct revelation to the mathematician alone, and his problem of communicating insight is a big one. Partly for this reason, our Company is building a special machine for simulating stochastic processes. This automatic Monte Carlo device will solve the most complex problem of this type without the use of algebra, very quickly (because it speeds up ordinary clock time by working to scale), and by visible operation will reach a visible solution. We call it SAM : the Stochastic Analogue Machine.

V — MANAGEMENT AND BALANCE

(a) The Idea of Homeostasis

The art of management involves, I suggest, an ability to balance. Not only must assets balance liabilities in the account we actually call a *balance sheet*; supplies must balance demand, services must balance production, and so on; in an organisational sense there must be a balance of authority, and personalities have to be kept in balance. Now balance is a highly scientific concept, insofar as in every field of science there is a special account of a balancing mechanism. In engineering, there are governers and gyroscopes ; in physics, there is entropy — the thermodynamic laws ; in chemistry, equations are the mode of description ; in biology, there is the stabilising of animal populations ; in cosmology, gravitation opposes cosmic repulsion ; in economics, genetics, psychology . . . and above all in physiology, there are many highly developed balancing devices. The list could be continued indefinitely, and it is not by mere chance that this is so. The whole of nature is in balance : its very existence is dependent on these complex stabilisers.

If the art of management demands the ability to balance, and if balance is a vital and universal scientific mechanism, then industry might gain from *models* of natural balance. Are the balancing devices built into industrial management anything like as well-designed and effective as their counterparts in natural science ? They are not. Think of the flow of information in a factory : it can be visualised as a herring bone. There is a main flow of instructions passing satisfactorily down the centre, and there are many offshoots which we might label : "to records". And that is the end of that. True, these records are sometimes consulted, and information from them is used in the managerial judgments which control future activity ; but this is a precarious, psychological effector. In nature, we find that this balancing mechanism is elaborately built into the system.

Engineers who build this type of mechanism into an individual machine call it a feedback. It is found, more simply, in the temperature control of an oven, where it is called a thermostat. A thermostat holds heat constant ; but I do not want to talk about heat, so I shall speak of a homeostat. Homeostasis is the function of holding any attribute steady against all kinds of disturbance and buffeting. A factory requires homeostatic control: it is a job for operational research to design this for management. In doing so, it will call on its scientific models ; any of nature's myriad homeostats may provide a suitable adaption.

When I spoke earlier of treating a works organically, it was probably not clear how this was to be done except insofar as interacting probabilities were to be taken into account. But now the real operational research tool for the task has appeared : it is **Scientific Analogy.** When an operational research department is described as including scientists of many disciplines (and they appear sometimes to be strange bedfellows), the reason is that we do not want to lose a valuable model in a given situation for want of enough learning to recognise it. And we do want the cross-fertilisation of scientific ideas.

In Part I we briefly examined a production control system which is self-regulating. In principle, it is homeostatic ; in practice, the homeostat will not be completely efficient until the feedback is made faster and more extensive, and this is receiving attention. When stock control was discussed, ways were found of determining the best levels of stock ; but no homeostat was described. Let us take that situation, which calls so loudly for this treatment, as an example.

Consider a certain class of work as it moves down the factory as enclosed in a pipe. The contents may be individual "lumps" (bits of steel, or engines, or pallets of components), but we shall pretend that the flow is continuous : the model is hydraulic. Material is drawn off from the end of the pipe as required, at arbitrary intervals and in arbitrary amounts. To keep up the pressure in this pipe a pump must be operated at the source, to draw more material up from a well. The well represents the production plant for the primary material, and the pump is the production plan. Now there is a device which *connects the tap to the pump*, so that drawing off supplies at

the finishing end has the effect of keeping pressure in the pipe constant. If the pump has other work to do, and the well other pipes to supply, it is no use having a simple connection between tap and pump: some sort of valve must be designed to smooth out the demand which is fed back to the source. All this can be visualised, and what is more designed mathematically. When the model has served its purpose it is discarded. We are left with a computing device (in the work we have done this is a small plastic gadget) which is operated by the consumer. He reads into the gadget the amounts he is drawing off the pipe, and the gadget (which incorporates the mathematics of the operational research model) tells him what orders to send to the pump — and those orders take account of the limitations on supply at the other end.

This is a homeostatic device; it is known to operational research as a *re-order rule*. Notice that it guarantees adequate supply, constant pressure, no exceptional demand on the producer or planning control, and above all minimum stock. For in this case there is *no* static stock; the stock is dynamic (I am tempted to say hydrodynamic) because it moves perpetually down the pipe; and the "size" of stock is simply a question of the pressure: that is, the frequency of the appearance of these components on route. When we discussed stocks before, I said that supplies could only be perfectly guaranteed by an infinite stock. Notice, then, how this homeostatic control effectively provides this Utopian service: since the supply is continuous, it is never exhausted.

These are two examples of homeostasis in industrial control, brought into action by operational research. Examples could be multiplied, but I will restrict myself now to the barest mention of this approach in the field of economics, in the hope that someone will be inspired to read Professor Tustin's book (8). Tustin has applied these principles to economic problems; he also draws on scientific analogy for the homeostat, but his model is specifically of an engineering kind: it comes from servo-mechanism theory.

Finally, while operational research is busy designing homeostats for management control, it is also watching closely for the homeostasis which is natural to the factory. For the organic whole of men-and-machines itself operates by self-balancing mechanisms, although they are not deliberately contrived. For example, if orders fall away in a works, the productive system reacts. A consciousness (which may hardly be recognised on the shop-floor, and should perhaps be called a subconsciousness) of lightened load develops; productivity begins to fall. The reason is that there is a natural compensatory tendency which seeks to spread the work and thus to avoid redundancy or short time. Most managers would endorse this interpretation. But, looking at this not as cussedness in the worker, but as a natural homeostat, what is the proper action? If falling productivity is met by the apparent economic safeguard of standing men off, the feedback employed by the management is positive, not negative. That is, instead of *correcting* the tendency to lower productivity, it will *increase* it. Now few managements have any scientific assessment of the correlation between productivity (note: *not* production) and profit. If that correlation is measured by operational research means, it might be found that the loss of money caused by the positive feedback in lower productivity is actually greater than the gain in wages saved by standing the men off.

This possibility is indeed profoundly disquieting. Perhaps it is not worth investigating in a mass production industry, where the pressure of the production line may enforce a reasonable rate of work and thus inhibit the natural homeostat. But most of British industry is not of this kind; it is mostly of a "jobbing" character. The techniques described can advise management on whether a proposed policy will in fact correct a dangerous situation, or make it worse. There is, I believe, a possible reversal of traditional labour policies in this approach which might have important social and economic consequences.

(b) Balance, Profit and Opportunity

Of the many other aspects of balance in industrial management that operational research can approach, I will mention just one. This is the problem of balanced production itself.

In a given works there may be a large variety of machines, of orders, and of possible process routes for each order. This situation, which may also be concerned with various methods of transportation to various alternative destinations, is a problem of choice. Out of the many millions of possible plans, which is the best? That is, how is the plant to be kept in balance within itself and with the market, all machinery fully utilised in the most profitable way? The fact is that in a normal factory, the production potential and the market demand cannot be in perfect balance for long; the factory's character cannot shape itself continuously and exactly to changing commercial and technological circumstances. Thus we expect to see some idle plant, some unprofitable business, some lack of balance at all times. Even so, at any given moment there must be a *best* balance amid the host of possible balances that may be struck. Discovering this best, or optimum, for any situation is called **Optimisation.**

There are various optimising techniques, and in a completely defined situation (that is, in a closed system) a uniquely most profitable pattern can usually be discovered. In practice, and despite the philosophy of organic analysis, very few problems present a closed system which is completely defined. The problem we examine is part of a larger system (ultimately, it is part of the whole world's economy); therefore operational research can only optimise over a given area, and after making some assumptions about the larger model into which this problem fits. Thus we are not strictly speaking *optimising* anything, and the approach usually made is consequently called by the even uglier but more accurate name of **Suboptimisation.**

The shop-floor planner (or the foreman) who is confronted with a number of orders and machines, and who uses his knowledge of their behaviour and

capabilities to decide what job to do next on each machine, is attempting a suboptimisation. In effect, he is turning over a vast number of possible courses of action, and trying to recognise the best. The human brain's mechanism for doing this is not well understood ; but two results are clearly established. The brain does fairly well with this problem : experience teaches, and the solution reached is rarely disastrous. But beyond a certain level of effectiveness, the brain ceases to discriminate ; it appears quite incapable of recognising the actual optimum itself.

Operational research has various techniques available for improving on this outcome. The most important of these at the moment is called **Linear Programming.** The use of the word " programming " explains itself ; " linear " appears because the operational research model consists of a series of linear equations. These equations link all the production resources with all their possible products. We use the mathematical tool of *matrix algebra* to produce an array of these facts. This array is a kind of tabulation in which the profitability of every machine is quoted for every possible product. The algebraic formulation links every possibility on each machine with every possibility on each of the others. That is, the model is made to stand for the whole range of alternatives which could be followed, and to include measures of the profitability throughout. Obviously, all these alternatives can no more be written out in full than the brain can contemplate every one of them separately. The beauty of this model is that it does include them all, without writing them all out.

Anyone who follows football closely is acquainted with permutable models of this kind. The model " five homes, five draws, two aways, basic line " will save the pool punter from writing out 16,632 alternative lines. All that remains is to pick out the winner. So, in our matrix of production possibilities, we have to pick the best, most profitable, combination. Mathematically, this means solving the linear programme in terms of a *maximising functional.* What does it mean to the industrial manager ? — simply this. It is possible to state what is the most profitable arrangement of available work. It is also possible to find the most profitable order book : that is, the work which the commercial organisation can most usefully *make* available.

This may not sound as exciting and valuable a prospect as it really is. The established levels of industrial productivity for current resources (which in heavy industry particularly include enormous overhead costs) can be considered as the best results obtainable. This is the status quo. Any augmentation of resources is then judged according to what economists call its *marginal utility* : that is (roughly) the return on its cost in these circumstances of established maxima. Normally, the marginal utility of capital in heavy industry is quite low. The exercise which selects the objects of investment in terms of linear programming, however, is deliberately exploiting weaknesses in the established pattern. The marginal utility of such directed investment may well reach several hundred per cent. : a fantastic rate of return.

The systematic use of this tool of operational research as a policy-making instrument could be effected through a special adaption of the costing system. For there is an obverse picture to the maximum profit ; this is the minimum cost. There is a linear programming theorem which shows that this suboptimisation technique reaches the same answer whether the approach be made through higher profits or lower costs. Now if we know the minimum cost of a course, in terms of a suboptimisation, then the difference between this and the actual cost of that course at present is the *marginal cost* of the present practice. A marginal costing system put in on this basis could lead management towards the ideal operation of its factory.

The final adaption of this technique is again through the criterion of cost. Suppose that a linear programme shows that a group of resources of men, plant and materials should be used to make product A for sale ; suppose further that to conform to some other policy these resources are used to make product A for conversion to product B. Then the opportunity of sale at stage A has been foregone. So the profit which could have been made by selling A should be included in the cost quoted for product B. This notion is called *opportunity costing.* One of the ways in which management could guide itself towards the ideal balance of operations is by costing in this way. In an industry (like steel) where operations down the factory consist mainly in converting previous products, it is often held that the profits all accrue at the finishing end. This impression may derive from the method of costing alone. A suboptimising programme might show that, on the contrary, some of the potential profit made in the early stages is in fact dissipated by foregoing the earlier opportunity of sale.

The techniques which I have briefly discussed in this section derive from econometric methods sometimes classed in general as *activity analysis,* and they may be studied in full in the book (9) edited by Koopmans. A very simple account of linear programming, discussing the costing aspects mentioned above and saying a little more about the mathematical model in elementary terms, may be found at (10).

VI — THE THEORY OF DECISION

(a) The Nature of Decision

What is an industrial decision, and who takes it ? Suppose that a sales clerk is told that a product has been wrongly made. Asked to dispose of the abortive result, he offers it to the only two customers who have ever shown an interest in such material. The cost of the product he knows to be £1,000. One customer offers £900, the other £1,100. So the clerk accepts the second offer without more ado. He has taken a decision ; or has he ? Is a clerk normally entitled to take a commercial decision involving such a sum ? Surely, what has really happened is that the

Sales Manager has *vicariously* taken this decision. The clerk has a certain delegated authority ; and he knows (that is, he is morally certain) that his chief would reach this same decision if so straightforward a matter were referred to him.

Many delegated decisions rely, I suggest, on this mechanism : the decider is reckoned to understand the principles upon which his superior would reach a decision. The effectiveness of this procedure must depend upon the efficiency of the mechanism by which the rules governing the decision are transmitted to the delegate. What is this mechanism ? Is there one ? How often has the decider-in-chief at the top even formulated such rules ? These are searching questions. The answers would seem to be that decisions are reached by acumen, and received by intuition. Now these two mechanisms can be most effective ; I do not disparage them, because they involve brain processes of great complexity which may be inimitable. But whether mechanisms which work brilliantly on policy problems between a general and his staff, or between a director and his managers, still operate properly at lower levels and on quantitative problems, is very doubtful.

Consider one of these quantitative decisions. Someone is told to run a piece of equipment experimentally, and to recommend whether it should replace the equipment at present in use or not. The criteria by which decisions like this are to be reached are often not thought out at all carefully. Firstly, the outcome will probably depend on how much time and money is spent on the trial. Secondly, it will depend on whether the right characteristics are tested, and whether those characteristics are measured properly. Thirdly, everything will hang on the " significance level " at which a decision is taken : is a saving of twopence per week a good basis for a change, or should it be £2, or £200 ? The manager who reads the final report may think he is taking these decisions. But he is not : such questions are almost certainly begged in the design of the experiment. The decisions may all have been taken beforehand by some junior who does not know the rules, and the answer may be a foregone conclusion.

With this background, operational research poses a number of questions. Are decisions being taken at the right level : not the *apparent* decisions, because everything has probably been delegated neatly according to the latest principles of management, but the *actual*, hidden, decisions ? Is the decision being taken according to its real nature, or on the basis of some popular superstition ? If someone does understand the real nature of the decision, is any mechanism available for transmitting its rules to the point of interpretation in action ?

We are now drawing for answers to these questions on both mathematical and logical methods. It is interesting and fruitful to understand the basis and source of these new and important techniques, which is very profound. It is a subject called *metamathematics* or, sometimes, *theory of proof*. This subject is concerned with the nature of meaning and decision at the level of philosophy : it is fundamental to our whole conception of life and the symbols we use to describe it. (It is also about the most difficult study that exists.) I mention this work for two reasons. Firstly, it is both the logical and the chronological basis of cybernetics, which we shall meet later. Secondly, it is worth noting that both logic and mathematics must be involved in such studies, and for what reason. Mathematics is concerned with the underlying soundness of its procedures and the underlying admissibility of its concepts ; logic is concerned with what can and cannot be said or demonstrated, whether mathematically or otherwise. Each subject brings its own tools to the job ; and the evolution of a common notation has much to offer for the future.

For our present discussion, however, we wish to draw on this work for some practical operational techniques. Methods which I shall call *Decision Functions* have emerged in both the fields contributing to the deeper mathematical research. A decision function is some formal expression which uniquely determines the rules for reaching a required decision.

(b) Decision Functions

A statistical decision problem arises when there exists a number of possible decisions, one of which must be taken according to a rule based on the outcome of some stochastic process (see Section IV (b)). The formal statement of this treatment is called a **Statistical Decision Function.** The sort of problem that can be handled is typified by the example given earlier about an experiment on some new equipment.

If this method were used, the experiment itself would be designed with care to produce the measurements really relevant to the decision. The course of the trials would also be planned in mathematical terms, and so would the set of possible decisions. The stochastic process is provided by the experiment itself, which is carried out within the framework of this design ; and the decision is reached for a given level of risk that it is wrong (on a vital experiment, this factor could be pitched very low). As a result, the outcome of the experiment could be given *as soon as* the evidence became sufficient to reach a conclusion. This is most important, because it saves both time and money ; it is usually impossible to recognise this moment intuitively, and even conventional statistical sampling normally takes more evidence than is absolutely necessary. Here we have the unique guarantee that enough evidence, and only enough, is sifted to reach a valid answer.

This is an example of a scientific decision. It is taken within the managerial framework of a specified problem with specified risks of error. Within that framework, for which the manager rightly takes his responsibility, the answer must be right. So far we have spoken of a simple decision problem, where a yes-or-no answer is required. The theory can be extended, however, to cover problems in which several courses of action are open ; none is " right " exactly, but one must be better than the others. In this multi-decision problem, use may be made of the *Minimax Theorem*. According to this, a solution is chosen which (mathematically) minimises the maximum risk. This sounds confusing at first, but it is a

model of the way in which people often take important decisions. They examine all courses of action, discover the most dangerous element that the situation offers, and then choose the one course which makes the risk of encountering this pattern the least.

It is not possible to investigate statistical decision functions further here, because we should soon run into heavy mathematics. These may be studied in Wald's book (11). But logical decision functions deserve further mention, mainly because logic (unlike mathematics) is a subject with which few people have any acquaintance whatever. The general method is known as **Symbolic Logic,** and I will give a trifling example of its use, mainly to show those who have never seen it before what the notation looks like. There is no stochastic element here at all: this method concerns the formal application of rules to which statistical risks do not apply.

Symbolic logic can take a set of rules, and turn them into algebra — propositional algebra. In this, the letters (*a*, *b*, *c*, and so on) stand not for *numbers*, but for *propositions* or statements. We might say that the letter "*p*" stands for the proposition "the furnace tapped at six o'clock". The connecting symbols between the letters are more difficult. In mathematics, we say that "+" means plus or add, "—" means minus or subtract. In logic we are concerned not with numerical relations, but with logical ones. So we say "$\vee$" means "either . . . or", and "$\supset$" means "if . . . then". To put a sentence like: "if the foreman comes round I will start work" into propositional algebra, we could say: let "the foreman comes round" be *p*, and let "I will start work" be *q*. The sentence can then be written: "$p \supset q$". And so on; there is an extensive logical notation to deal with the most complex ideas.

The reason for expressing an argument, or a decision function, in these symbolic terms is this. In mathematics, once a set of numerical ideas has been put into algebra, the ideas themselves can be forgotten — they are confusing. There is a special body of rules for manipulating the symbols in the algebra so that the right answer is guaranteed. ($a^2 - b^2$), for example, may be replaced by $(a + b)(a - b)$. So, in symbolic logic, there is also a set of rules for manipulating the algebra; this also guarantees that the right conclusion is reached without thinking about the ideas behind the symbols. Here is a simple example.

Two firms, Forgem and Stampit, are branches of the same company. A customer of both firms puts the same enquiry to each, and both are interested. If Forgem does not take this order, it has no alternative outlet for the product concerned; but Forgem is in temporary technical trouble — it may have to decline the order even if it can find no other outlet for this product. However, once Stampit takes on the job, Forgem may never be offered the chance again. So the two commercial managers agree that if either Forgem and Stampit both accept the order on the one hand, or Forgem neither accepts it nor finds an alternative outlet on the other hand, Stampit will take it on if and only if Forgem does find another outlet for the product. This is perhaps the sort of arrangement people do reach over the telephone. At the time they may even both understand it. A week later, Forgem has to give a snap decision. It has just concluded that it cannot evade technical trouble on this product, and it *has* found an alternative outlet, so the Forgem manager wants to decline the order. He has not had time to talk to his colleague at Stampit. Can he go ahead, or will he break his agreement?

In this story I have muddled the facts sufficiently that most of the people I have asked cannot give the answer without a great deal of thought. In fact, there are only three basic propositions involved; let us name them symbolically. Let:

p = Forgem accepts the order
q = Stampit accepts the order
r = Forgem finds an alternative outlet

Now we can translate the agreement into a logical decision function:

$$pq \vee \sim (pr) \;.\; \supset \;.\; q \equiv r$$

That is the agreement, in symbolic terms. (The dots divide the function into logical components, and the symbol "$a \equiv b$" means "if *a* then *b*, and if *b* then *a*".) Now in the circumstances given we already know the "truth value" of two of the three propositions. The truth value of *r* is truth (because another outlet has been found), so we replace *r* by $\top$ (for "true"). The truth value of *p* is falsity (because Forgem wants to decline the order), so we replace *p* by $\bot$ (for "not-true"). Then, using the rules of this algebra, we simplify the expression, performing what is called a *truth-value analysis*. Like this:

$$pq \vee \sim (pr) \;.\; \supset \;.\; q \equiv r$$
$$\bot q \vee \sim (\bot\top) \;.\; \supset \;.\; q \equiv \top$$
$$\bot \vee \top\bot \;.\; \supset \; q$$
$$\top\bot \; \supset \; q$$
$$\bot \; \supset \; q$$
$$\top$$

It would be out of place to teach the rules of the algebra here, but I think you will be interested to see what this analysis looks like. The decision function has been reduced to $\top$ (true), without specifying the truth-value of *q*; that is, without assuming what Stampit does about the order. So the consistency of the rule is established independently of Stampit's attitude, and the answer to the original question is that Forgem may go ahead and decline the order without more ado.

Throughout this Paper, my accounts of techniques must be introductory; this is nowhere more keenly felt than in these notes on decision functions — a subject of great breadth and intricacy. I can only hope that the impression remaining will not be limited to a trite view of the problem of experimental design, and an amused recollection of the (quite apocryphal) dilemmas of sales managers and their (rather improbable) solutions. The serious enquirer who is willing to come halfway to meet me, if he

pauses here, may well glimpse the potential importance of this group of methods in a great many spheres of application.

(c) The Game of War

The third question to be discussed under the heading of Decision Theory introduces an historical note. It is well known that operational research originated (at least, under that name) in the War of 1939 - 1945. A main concern then was with *strategy,* with the application of scientific method to problems of what to do and how to do it when faced with an enemy. In this situation, many of the problems already examined here were met: problems of measurement, of probability, of stockholding, of queuing, of balance. War, it might be agreed, is a grand imperial military stochastic process. Indeed, it was precisely because these were the problems met with in war, and moreover because these problems proved amenable to operational research treatment, that operational research spread to industry in the first place. For, argued those involved, industry presents many of the features of a battlefield. So many of the practical problems encountered in both places depend less on any "enemy" than on the arbitrariness of nature, on chance and mischance.

Many problems so depend: but not all. For in war there is in fact an enemy. And in industry perhaps there is an "enemy" (I use inverted commas) as well. At least, if the model of war proves useful, it is easy to set up an enemy for operational research purposes. What is the point of so doing ? It is this: the feature that an enemy introduces into a problem is the strategic feature that someone else, whose hand is not disclosed, is actively trying to upset one's plans. A military commander (a manager) may wish to stockpile shells (products) in order to lay down fire (tackle a market). The size of the stock will basically depend, as we saw earlier, on the stochastic processes of armament supply (manufacture of goods) and expected rate of fire (sales). But now there happens to be an enemy (competitor) with a good intelligence service (market research). He plans his attack or forms a strategy based upon the desirability of taking *his* action when the other side's stocks are low.

So an enemy with an undisclosed strategy may very well figure in an operational research problem in war or in industry. I trust it is clear that the term "enemy" is used as part of a model, and not as exemplifying a "jungle" approach to industrial affairs. It may be convenient to call a competitor an enemy for these purposes, or even to put a customer in this role. After all, while the supplier and the customer may take each other out to dinner, and be very frank about many matters, each may well be nursing a strategy which he conceals. None of this is very lethal, perhaps ; but the model holds.

To tackle a problem of this kind, that is one involving strategies, operational research has turned to the simplest possible model of a strategic situation. This is a game ; a game, say, of poker. This would be a logical and praiseworthy thing to do ; but I will not disguise the historical fact that it was something of an accident. One of the world's greatest living mathematicians, J. von Neumann*, has been investigating the nature of games mathematically — as early as 1928. He and his economist collaborator gradually built up a monumental treatise (12), which was published in 1944, in which games were carefully analysed both in their own right and in their capacity as models of economic situations. When the wartime strategic problems appeared, and later industrial problems of strategy, this work proved to be the basis for much advanced thinking. It is called the **Theory of Games.**

The simplest game of all is known as a *zero-sum two-person game.* This merely means that there are two sides involved, and not a number of independent contestants, and that the result of the game is to redistribute rewards rather than to create them (that is, the sum of payments is zero ; if A loses sixpence, B gains it). This is worth mentioning partly because it is a byword to all students of game theory, and partly because it offers a peg on which to hang some thoughts that were entertained earlier. Firstly, note that any linear programme can be expressed as a zero-sum two-person game. Secondly, note that the "decision problem" can be interpreted in the same way. Thirdly, the Minimax Theorem mentioned earlier offers the solution to a game where the strategy required must minimise the maximum risk. There is a certain unity in all this work which encourages optimism: this unity lies, I consider, in the theory of sets (mathematics) or the theory of classes (logic) which is the common basis of each of these techniques, and is so deeply founded in the metamathematics spoken of before.

From all this work emerges, first of all, an understanding of the mechanism involved in dealing with either an enemy or an "enemy". The student of game theory has an insight into such strategies as bluffing and the formation of coalitions. The best means of applying this insight quantitatively will gradually become more obvious: some excellent results (such as linear programming supplies) have already been obtained. Much will depend on management's ability to see a problem as a "game" situation, and its willingness to set operational research to work on it. But in looking into the future for these applications (an object set by the title of this Paper) I will consider finally a possibility suggested to me by a most interesting footnote to game theory supplied by Braithwaite (13).

From the start I have tried to emphasise the essential neutrality of operational research, its scientific objectivity. In what industrial field, could one say, is this calmness and this dispassion most required ? Surely, in the field of labour relations. All too often, unfortunately, the management and the unions are "the two sides of industry"; absurdly enough, they are "enemies". Here, in fact, is a two-person game which is all too often zero-sum, although it need not be. Suppose that a specific problem arises, and that a deadlock is reached. Both management and union have a strategy. Now let

* Since the preparation of this Paper, news has been received of von Neumann's untimely death.

us make two more suppositions : I do not know which is the more difficult. First, suppose that an operational research man can succeed in adapting this real-life problem to a game form ; second, suppose that both management and union are willing to hear him.

First of all, our man sees the management. He points out that, in default of union co-operation, the only safe premise is to assume that the union will adopt the strategy which most favours their members. In the light of this union strategy, he can demonstrate the best management strategy ; *and he can evaluate it* in terms of units of reward. (This is called, by the way, the " pay-off matrix ".) He then sees the union, and, pointing out that management is likely to adopt the strategy which favours it most, carries out another similar exercise. The union pay-off matrix is also evaluated. Both parties ought by now to be convinced of their own best strategies, assuming an " enemy ". Both parties will also have a scientific guarantee that the maximum pay-off of their strategies is a certain evaluated figure — let us say 100 units in each case.

Now comes the joint meeting. The operational research man reminds each party of these results. He reminds them also that when parties collaborate, instead of opposing each other, each is likely to gain from the collaboration ; they will recognise the truth of this, because each is always telling the other that it is indeed the case. Abandoning the two " enemy " strategies, the operational research man then tackles the " game " afresh, and evolves the *optimum strategy* for the game as a whole. This assumes that each party behaves as the optimum strategy requires, and aims solely to get the best result out of the game. There is little doubt that the evaluation of the pay-off matrix will now produce a better figure — say 160. This means that, by collaborating, each party draws a bonus, which Braithwaite calls a " collaborator's surplus ", of 60 units.

In a way this approach is not new. It is a form of conciliation which calls on both parties to accept the benefits of a joint approach. Please note carefully, however, what *is* new. Firstly, each party has a *numerical estimate* of the value of a private strategy *proved* to be the best. Secondly, each party has an assurance that the joint strategy is also the best, and that his part in this is (putting it bluntly) *the best dividend he can possibly draw from the situation.* The point is that this solution is in no sense a compromise. It is quite definitely, for each side, the achievement of its best possible result. Nobody is preaching ; nobody is preached at ; nobody is having to pretend that " the country comes first " (it does, but there are too many players to bring into that game). All anyone is asked to do is : (a) acknowledge the facts ; (b) acknowledge that there is another side, probably with a legitimate point of view, which cannot just be ignored ; (c) agree that once a neutral scientific evaluation has been made there is not much point in either name-calling or trying to improve on the mathematics which *are* the situation.

It may be that these thoughts will be considered naïve, but that is a risk I gladly take. If this method could be worked from the technical point of view (and it would mean choosing the right sort of problem), the potential benefits to industry and to the country are inestimable. This is why I have suggested this approach ; this is why I have not tried to " wrap it up " in the least. There are hard facts in these dangerous situations, as everyone knows, and the gap between speech and action is one of them. If science is to come into the field it will face up to facts coldly, and not two-face up to them. Again I repeat : the scientific method is neutral.

PART III
CYBERNETICS

VII — PLANS AND PROMISE

(a) Why bring in Cybernetics ?

It would not be strictly correct to call operational research *a science* : it is a corpus of scientific techniques. On the other hand, a group of highly developed methods, centering on a common concept, is likely to crystallise into a science. I consider (although this is a personal judgment) that operational research is crystallising in the science of cybernetics. Firstly, then, let us look for a common concept which runs through operational research as described in this Paper.

As far back as II (c) the notion of feedback intruded, and an inanimate system was endowed with " liveliness " by a metaphor. In III (b) came " life-like-ness ", as applied to factories which were to be regarded as *organic wholes.* Stochastic processes were examined in IV (b) : and these are the randomly-unfolding but goal-directed activities of nature and its organisms. In V (a) we reached the notion of homeostasis, which is a concept distilled from the balance of nature itself. Then the problems of planning and optimising were discussed in V (b) in terms of an attempt to reproduce mathematically the technique used by the brain for these purposes. By the stage VI, operational research techniques were being examined quite specifically as alternatives to human thought processes.

Thus the view of industry as something living, or something operating as if it were alive, took shape. This is the *cybernetic model.* Cybernetics is, in fact, the science of communication and control ; but it specifically attends to these features *both* in machines *and* in live animals. Each is a model of the other. Thus, by this year, which sees the 10th anniversary of the coining of the word " cybernetics " by Norbert Wiener, much progress has been made. Animals have been studied in the light of our knowledge of machines, and particularly in the language used by scientists outside the biological fields. Conversely, machines have been studied by methods founded in the biological sciences, and some mechanical copies of living processes have been made. What tools have been used for this work ? Well : mathematics and statistics, the theory of probability, models of various kinds, stochastic processes, logic . . . I need say no more.

It does appear that operational research methods are specifically appropriate to cybernetic studies. This is not really surprising, since nothing is less static or more *operational* than a live process. And cybernetics itself will, I am quite confident, add a new dimension to industrial thinking. This is why the techniques and the science come together as the climax of this appreciation of the scope for operational research in industry. The technical basis for their interconnection is examined more thoroughly in another Paper (14).

(b) Cybernetic Machinery

Machines as they are normally understood in industry imitate animals to the extent that they perform actions. They have a source of energy, and they act — under an operator's control. As machinery has evolved, so it has tended to become more automatic. Animal homeostasis was imitated to the extent that automatic cut-outs, governers and stabilisers were fitted to machines. Eventually, a species of automaton has been developed: by this word I mean a machine which imitates, in a fully determined way, the animal's ability to operate *itself* over quite a range of activities which it was designed to perform. Automation has already delivered the latest members of this species of automata: there are transfer machines, guided weapons, and electronic computers. All these are automata, and all depend upon the notion of feedback as a means of self-regulation.

This is the beginning of cybernetics. But automata are still nowhere near as large as life, and for one outstanding reason. They are predetermined. The designer can always (in principle) state, rather than predict, their behaviour in a given set of circumstances. The owner of a rat can predict its behaviour insofar as he can set limits to it: his rat will not suddenly fly out of the window, or build a bridge. But even so lowly an animal has freedom of action within those limits. If it is put into a maze it will wander about. If it is presented with a choice between two passages, no one can say which it will choose. In other words, its behaviour is stochastic. An engineer who built a machine whose behaviour was stochastic and not predictable would probably be certified.

Nevertheless, it is to stochastic machinery that cybernetics is turning. The reason is simple. In nature, stochastic exploration of a problem by, say, a rat leads to conditioned behaviour. At the end of one passage in the maze is an electric shock ; at the end of the other is cheese. After a number of trials, the rat *learns* to avoid the shock, and to reach the cheese. After millions of years of evolution, nature finds this sort of behaviour the best mechanism for coping with an unpredictable environment. Again, the reason is obvious. If a rat were born with a built-in programme for running that maze (that is, if it were an automaton), it might then find itself in another situation altogether and thereby starve.

So cybernetics has studied the rat, and its stochastic behaviour towards conditioning. Mathematical models of the process were constructed, and the concept of a "statistical rat" (which the Americans call a stat-rat) was created. To build a machine to this model was the next step. And there it is: a machine that will actually *learn*.

This is cybernetic progress, and it will lead to a species of machine that can imitate a whole new range of animal (including human) behaviour. The ability to learn, to recognise complex patterns, to discriminate, even, perhaps, to amplify the intelligence of its master: these activities typify true cybernetic machinery. Grey Walter has already put this branch of cybernetics before the Institution in his Paper on "Thinking Machines" (15).

To return to the key concept of homeostasis, however, I must refer to The Homeostat itself, the invention of Ross Ashby (16). A truly cybernetic homeostat will not operate just like a thermostat, clicking on and off at preordained temperatures. It will seek a preordained stability, it is true, but its route to stability will be stochastic. Ross Ashby's machine does exactly this ; it has no industrial application yet, to my knowledge, but it is a pioneer piece of work. So is the mathematical model to which it is built.

(c) The Basis and the Prospect

It is not my aim here to give a complete guide to cybernetics, but simply to state what it is all about, and where it fits into the picture. Perhaps the point is made: the control of complex industrial situations, such as those discussed earlier, can surely become a reality through the machinery which can bring the operational research models of life *to* life. But it is in conclusion possible, in view of the ground already covered here, to look quickly at the really basic notions which make the subject so challenging and so promising. These derive from the sort of work described in VI (a).

Many years before the word came into use, cybernetics became a reality in the studies of meta-mathematics. Briefly, the story is this. A great British logician and mathematician, Turing, published in 1936 a proof that it would be possible to construct a machine which could reproduce itself. This work is not a piece of engineering, but a piece of meta-mathematics: Turing was concerned not with construction, but with *logical* possibility. Since then, the work has been extended. We now know that a machine can in theory be built which would produce other machines *more complex* than itself.

This guarantee is the golden key to the cybernetic future. Mankind has never really believed in this possibility, outside the realm of fiction. The reason is perhaps that mechanical instead of biological thought processes, or models, were used to consider it. Since a machine is always less than perfectly efficient, a self-reproducing machine must be impossible. It savours of "perpetual motion". But this is a misleading approach ; it is, in fact, the wrong model. It is not *energy* that we envisage conserving within the machine — that must be supplied. It is *design* which must be conserved or extended, and this is another matter. In the biological sphere, it is at once apparent that natural, vital,

things evolve. They evolve, as you and I evolved in the course of our own development, *from the less to the more complex*. This is the cybernetic model.

It is from this theoretical basis that the stimulus comes for concentrated cybernetic research. Cybernetics is not just a facile model for operational research in industry ; it is a new scientific and industrial target of immense potential. I say with sobriety that cybernetics will ultimately join atomic energy in reshaping the world. And operational research is industry's best tool, indeed its only mechanism, for working towards this future.

The results will come by stages : by all means let us come down to earth. So far, the main gain has been from cybernetics to operational research, in the provision of a potent model. We are now engaged in putting the model into useful industrial form in systematic terms. Initially, that system is controlled by hand ; it has built-in decision functions and so on, but human brains operate it, and the learning and discriminating and pattern-recognising goes on in human brains. The next, and current, step is to operate the system automatically. Our research is dedicated to the elementary stages of the final phase : to replace the more important brain functions in the system. This will not have the effect of making the brain of man redundant. It will : (a) improve industrial control beyond recognition, because the brain-analogue, or cybernetic machine, can be perfectly adapted to its task — without strain, without fatigue, without boredom ; and (b) release man from these tedious tasks for better things.

The first faltering approach that we have yet been able to make to these developments is discussed fairly fully in another Paper (17). These are early days, but full of promise and excitement. However, the prospect is clear before us ; and the drawing together in a single operational research team of scientists expert in subjects as apparently disparate as neurophysiology and electronics, mathematics and psychology, may now be more sympathetically understood.

FAITH, HOPE AND PROBABILITY

This review is now complete : it has ranged widely over the field for the industrial application of operational research. Many techniques have been introduced, and with them quite a large number of technical terms. I fully expect that these new ideas will spread themselves so widely, and with such impact, that informed people in industry will sooner or later want to recognise these terms in speech and in print.

I am asking much in presenting so long and difficult a Paper : the case must rest on a claim for the importance of this approach. British industry is without doubt a proud and worthy institution, but one perhaps seriously encumbered by its very Britishness. Some typical phrases resound in the ears of operational research workers. " It may work there, but this place is different " (but is it ?). " What about the human element ? " (what about it ?). " I am a simple sort of chap " (he never is). " There are too many variables " (abdication, this). And so on : we fall back on the reputation for muddling through. As an operational research man, my affectionate description of this industrial scene is : faith, hope and probability.

And yet there is much which really worries us. Britain falls desperately behind Russia and America in the productivity race : they have discovered that quality and quantity are not (as we so often suppose) mutually exclusive. Some strange blight has turned a war well won into a Pyrrhic victory : we are outpaced by those we vanquished and laid low. An Englishman rather naturally turns for his harrowing example of this to ships. For all time (excepting only wartime in America) Britain has led the world's production. In 1956, while we launched less tonnage than in 1954 and 1955, Japan more than doubled her output and snatched the lead. Germany is now very close behind. Thus Great Britain forfeits her historic leadership and the proud title of the world's shipbuilder. We have heard the official explanations; but where is the cry of anguish ?

It is this typical absence of alarm or even concern that prompts me to end on this note. Our problems are obviously great, and I should say unique in the world. We cannot afford to be mesmerised by soothing spokesmen, nor to be taken in by our own advertising. In invention and design we may still be among the leaders, and much of our plant is superb. But those who take too much pride in this are ignoring the international facts of trade. They are proponents of smugness. There is a new dimension of industrial management to be discovered : the cybernetic dimension, the dimension of *liveliness*. There are all the components of a body to hand : they await techniques of assembly into an organic whole. Britain can lead in this effort too : then her industrial economy can get up and run.

Now this is a diagnosis, and not a cure. It is not for operational research to resolve the country's predicament, and I am not selling a panacea. kinds : political management at ministerial level, Management alone can do the job, management of all administrative management in the civil service, line management in industry itself, functional management in engineering. But it is, I propose, significant that operational research has an approach to offer management of every kind, and a set of powerful tools which are *uniformly* applicable. This may help not only to solve the problems, but to supply the need for cohesion.

It is sometimes held that operational research men are simply people with critical intelligence and the time to think : they are employed by managers too busy to stand back and think for themselves. Provided that no one ignores the special orientation, the mastery of analogy and probability, and the battery of special techniques which characterise operational research, this definition will do. But " time-to-thinkers " must come out of their brown studies to convey their thoughts. This I have tried to do in the course of this exposition, and my thought is of a great industrial emptiness : the absence of organic control. If some sense of this void has been conveyed,

that is well. If some awareness of the new dimension in which such control could subsist has been communicated, that is better. And I cannot hope to convey the excitement that I personally feel as we move into this new dimension.

Finally, I thank the Institution for a neutral platform from which to put forward the need for a neutral platform in the shape of operational research. It is not surprising that a visitor's eye should fall upon the motto of his host: "*vires acquirit eundo*". The fact that the Institution has invited this Paper may excuse my asking for a momentary share in its snippet of Virgil on behalf of operational research. Our subject, like yours, "gathers strength as it goes".

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Two books dealing with Cybernetics, one from a theoretical angle (though in most lucid style) and the second from the point of view of historical development and gadgetry, are :—

Ashby, W. Ross (1956)—*An Introduction to Cybernetics,* Chapman and Hall.

de Latil, Pierre (1956)—*Thinking by Machine,* Sidgwick and Jackson.

REPORT AND DISCUSSION

Chairman: Mr. E. W. HANCOCK, M.B.E., M.I.Mech.E., Hon.M.I.Prod.E.,

President of the Institution.

The President, on behalf of the Institution, extended a hearty welcome to all who were present. Many of them, he said, had come long distances in spite of petrol rationing and other clouds hanging over the country; and this was appreciated. He expressed a special welcome to Mr. F. T. Nurrish, M.B.E.; Joint Managing Director of George Bray and Co. Ltd., who had had much to do with the present occasion.

The Institution had a series of Named Papers, and this was one of the most important. These Papers were presented throughout the country. In 1952 the Directors of George Bray and Co. Ltd., donated £500 to the Institution to be invested so that the interest each year would finance the presentation of a Memorial Lecture in remembrance of Colonel George Bray, who made a continuous contribution to the work of the Institution during his lifetime and was at one time the President of the Yorkshire Section.

The terms of reference for the George Bray Memorial Lecture stipulated that the subjects selected should be those not traditionally associated with production engineering. This was entirely in keeping with the Institution's policy of broadening the base—growing up and broadening. The present lecture was the fourth. In 1953 there was a lecture on "The Manufacture of Plate Glass," by Sir Harry Pilkington; in 1954, Mr. Robert Douglas gave a lecture on "Production Aspects of Synthetic Fibre Manufacture"; and in 1955 there was a lecture on "Pride in Workmanship: Today's Challenge" by Sir Gordon Russell. The subject of the present lecture was "The Scope for Operational Research in Industry" and it was a great privilege to introduce a young man who had obviously made the maximum use of his time, as could clearly be seen by reference to his illustrious background, details of which appeared on the first page of the Paper.

This was a very brief résumé of the life of a young man who had gone a long way; and it was a pleasure and privilege to introduce him and to ask him to give his paper on "The Scope for Operational Research in Industry."

Mr. Beer, in presenting his Paper (which appears in full on pages 298–320) said: I have been doing a little statistical prediction, and I find it would take me some three hours and nineteen minutes to read this Paper. I should like to let you into a secret about the way in which operational research works with the managerial staff. We have to be very careful indeed to make sure we do not set up reactions in the people we are trying to help; to make sure we do not upset them in advance. Therefore, I do not propose to read this Paper.

I propose instead to develop some of the ideas in the Paper more or less as they come to me, in the hope that you have read it—or, if you have not read it, that you may be stimulated to do so later on.

I should like to introduce the idea of operational research, which I can do in speech much better than I can do in print, by reference to a very lighthearted history of the way in which the subject has come about. But do not be misled by the lightheartedness, because I am trying to bring out four major points about the way in which this subject operates.

If we look at the history of operational research and ask ourselves where it started, I think we shall probably have to go back to the Second Punic War in the year 213 B.C. Here we find the Roman Fleet besieging the City of Syracuse. And what happened there? Well, the King of Syracuse was stuck for the solution to his problem. His people were being starved out, and he called in a scientist to help him. This scientist was Archimedes. There have always been military scientists. If we go as far back as we like into history, we can always find people specialising in military science. Indeed, it is probably the first recorded scientific method we have.

But at the back of this incident lies one interesting feature: that Archimedes was *not* a military scientist; he was a mathematician. And my reason for bringing in this incident is that it does emphasise one of the primary features of most operational research work: that we use what are apparently *irrelevant* sciences. We use sciences which are not obviously applicable to the work we are trying to do.

We expect to have in steelworks men who are scientists in the field of engineering; men who are scientists in the field of metallurgy. We do not necessarily expect to find a biologist in a steelworks. But we have found that by using techniques borrowed from apparently irrelevant sciences we are able to develop a better understanding of the processes we are trying to control. That is my first point, and it goes a long way back.

The "Square Law"

Let us come a lot nearer now—to the First World War, and to a man called Lanchester. Again he was a mathematician; he was not a military man. He was very impressed by what he saw of the First World War and he came to think: "Well, I wonder if we could not develop a scientific approach to the problem of strategy." This was at the time very surprising indeed, but he set to work and produced some mathematical models of military strategy. One thing, for instance, which I could mention, because it is very neat and easy, is that if you are outnumbered by an enemy by, say, two to one, you must have four times his fire power in order to establish an equivalent chance of winning the battle. That is the "square law" and is an example of the sort of thing Lanchester developed.

Just as happened in the case of Archimedes, who was killed off after all his efforts, it is the case here too that very little notice was taken of these ideas of Lanchester's until a generation later. But then they *were* taken notice of, in the last war, and they proved very useful indeed.

The second point, then, is that in operational research we have a very basic idea indeed—the idea that you can construct a model of a situation which will tell you something about the way the situation works. My first point, therefore, is that you use irrelevant sciences; and my second point is that you use models; these two thoughts go a long way to explain to you what this is all about.

The third point I should like to mention concerns *variety;* and here I am coming up in my history to the man who is associated above all others with operational research today—Professor Blackett. Professor Blackett started O.R. work in the

anti-aircraft field, the idea being that you could not hit a target you were supposed to be able to hit with the predictions available at the time. What was first of all important in that class of work was this: that it was recognised that there is a difference between theory and practice. And this is, I suggest, one of the primary points about operational research and the thing above all which distinguishes it from laboratory investigation. Operational research exists precisely to investigate the difference between theory and practice and to do something about it. So if your reaction to my Paper has been that it is a bit "airy-fairy," a bit high-flown, please believe me that we are quite fundamentally concerned with this problem of the real world. We are people who are worried—very worried—about the way in which nothing in practice ever seems to match up to what we expect.

That is one idea that emerges from Professor Blackett's work, and he went on to find out what was going on in the *practical* War. In doing that, he found that he had to call on irrelevant sciences, as I said at the start. He had to call on all sorts of people and to form a team of scientists from many fields. This team became known in the War Office and elsewhere as "Blackett's circus." And well might it have done!

That is why we have come to expect to meet in operational research people in all sorts of peculiar walks of life. Just what are they doing? They are experts in models; they are experts in recognising in a situation something they already know all about in another field altogether. Now that will exemplify, turning back to my history, what I am saying about variety.

We can call to mind academic men who came into research early in the late war. Professor Blackett was a physicist, and his involvement was not surprising. Someone who *was* surprising was Sir Solly Zuckerman, because he was an anatomist. You might say, "Why did he come into the picture?" He came into the picture in terms of civil defence. He came in to advise the Government on air-raid precautions. By the application and use of models from anatomy, he was able to predict with incredible accuracy the result of the 500-bomber raid on Coventry. He took a lot of the sting at the time out of Governmental and Service fears about the possible damage that would be inflicted by mass raids. He afterwards became scientific adviser to the then Premier, Mr. Attlee. That is a bit of background as to how people came into the O.R. field.

More and more people came in, and there was more and more variety, and that brings me to my fourth and last point. Here I should like to give you a little case history concerning Mr. E. J. Williams, who found himself in Coastal Command in the early War; and this will demonstrate my fourth point. (You will remember that the first point concerned irrelevant sciences; the second concerned models; and the third concerned variety and the difference between theory and practice.) The fourth is *surprise*.

The Surprise Element

What do I mean by surprise? I think it is hardly surprising that the application of this completely new line of thinking should result in recommendations which are surprising to the people who receive them. It is important to recognise that, because insofar as we are using unusual techniques, we come out with some rather peculiar ideas. People ought to expect that and, indeed, it is one of the exciting things about O.R. that this new approach, this new slant, may give a result which no one has previously thought of at all. If the recommendations are surprising, what may be the results? I want to tell you about the surprising recommendations and the surprising results that were produced by Mr. Williams.

The story is briefly this. First, the Coastal Command crews became accustomed to spotting enemy submarines and dropping depth charges on them. They dropped them on the submarines as they submerged; yet the results from the bombing were disappointing. There were too few kills. The depth charges were set to explode at a depth of 100 feet or more. This was presumably done as a result of some nice theoretical thinking. And here is the difference coming out between theory and practice. According to the theory it should be possible to explode these depth charges with telling effect, but it did not come off in practice.

The result of Mr. Williams' investigations—and here the surprise comes in—led him to recommend that the depth charges should be exploded at a depth of 25 feet. That is a long way from 100 feet; but even more surprising really because the scale of depth charges only went down to 35 feet before. That was the limit of what was considered reasonable. Someone at any rate was fortunately not overcome with surprise at this recommendation and it was put into effect. The result, according to the records of the Royal Navy, was an increase in kills of 400% in enemy submarines. The Royal Air Force figure was 700%. You can come to whatever conclusions you like about that, according to whether you are optimistic or pessimistic. But there was a result that was a considerable surprise.

That is how it all began. Do remember—the irrelevant sciences; the models; the variety, and theory and practice; and the surprise—my four points.

Now, in industry we O.R. men find ourselves very widespread throughout the country, working to create operational research teams to do this sort of thing in industry. Why? Because of the clear similarity between military operations and industrial operations. People sometimes boggle at this, but I really fail to see the difference. There is such a tremendous similarity between the two. In each case, management in industry and the strategic command in battle, we are concerned primarily with chance and with mischance—much more than with predictable and nicely set out facts. Anybody can see that this is the sort of thing that happens. Is the enemy coming up behind you where you are not expecting him? Is the furnace going to blow up and you are not expecting that?

Unity of Thought

As I have said in the Paper, one of the objects of linking together all these specialists is not to have a lot of people who all know a bit about something, but to try to mend the rents in the unity of knowledge which philosophers down the ages have seen to be fundamental to human thought. There is this terrible specialisation which cannot be avoided; unified *operational* thinking is enlarging our understanding of nature and of life. A lot of things that we are beginning to find out by operational research certainly—I think you would agree—come into the category of things which have fallen between the stools of specialisation. For instance: the physicists said there was no difficulty about something, but it was not really their field. The mathematicians said the same, and so did the biologists and the engineers. The result was that the thing, which was the concept of a brain-analogue machine, got lost. Cybernetics is essentially the end of all that.

I underline the word *operational*. That is the clue; that is the whole idea. We are trying to find out how things work in practice to get to the *real* world, to get away from the recondite, the theoretical. I would ask the people who have not understood this at least to believe that that is what we are after all the time.

Now, I want to turn to what I call in the Paper the basic techniques, the basic methods, the basic approach to the subject. If one sits back and thinks about it, it centres in the idea of *alternatives*.

I expect you know the little story about the girl's father who tackled her young man and said: "My boy, I want to know something. Are your intentions towards my daughter honourable?" And the young man replied, "Oh, so you mean there is an alternative?" That story goes to show that very often the alternatives that are open to people in an operational situation are not recognised. I do not think I am going to have trouble in selling that proposal to you. You all know how often in your works it happens that something has been done so long that, without any discredit to anybody, an alternative idea just is not visualised at all.

Operational research tries to specialise in spotting these alternatives, and having spotted them to find out whether there would be better methods, better control, or whatever it is, than the thing that is already in being.

To do this, the first things we must have are commensurable measures. The stage has been passed where one needs to sell the idea of measuring. We all know production requires to be carried on in a scientific fashion. You have to quantify always. The point I want to add to that idea is that we need to have *commensurable* measures. That is the point where we are sticking. We know we have to measure. We have thousands of figures round our works today. But we do not know how to compare them. Some are in tons; some are in money; some are in this and some are in that. We have various ways of bringing these things together, of standardising them, of making them equivalent. Some of these techniques work, and some of the figures can successfully be made commensurate.

Enjoying the reception preceding the Lecture are *(from right)* Mr. Stafford Beer ; the President of the Institution, Mr. E. W. Hancock, M.B.E. ; Mr. S. R. Howes, O.B.E., Director, The United Steel Companies Ltd. ; Mr. J. R. Widdowson, Chairman, Sheffield Section ; and Mr. A. A. Francis, Chairman, Institution's Papers Committee.

That is the first point: we must have commensurable measures. The second point arises from the first point, because if you are going to have commensurable measures, you must have the right model within which to operate these measures. In other words, a single transformation such as we now have in applying a mere formula to make two things comparable (such as francs and pounds) is usually not adequate for transforming, for instance, a tonnage in a steelworks in one department to a tonnage in another department and comparing the outputs of the two departments. The reason for that is that there are a lot more factors that come into the thing. There is a different " shape "; a different process with a different *mystique*. It is the rightness of the model, which I have been compelled to call isomorphic—because I do not know another word for it. I am sorry about that, but once you get hold of the idea it should be all right. It simply means that you have to have the same shape, the same form, in the model as in the situation.

This enables you to construct a model which operates in the same way as the situation. It is just a microcosm; it is an operational copy of the actual situation.

Why is it important? It is important because it gives an *organic* idea—the idea that the thing you are studying operates not in a lot of little bits which you can study and change, improving this bit or improving that bit, but as a *whole* where you have to understand what is going to happen in general if you alter one of the bits. That is the thing we have fallen down on in some ways. We are trying to improve " a bit," and we get all sorts of repercussions that we do not really understand. Therefore we must have the right model of the organism as a whole.

Now we often talk about this idea of the organic factory. We are used to the idea of the organic factory. " But," you will say to me, " this is a lot of hoo-ha. I have heard all this before. We *know* factories have to operate in an organic way. We *know* factories have to be integrated. That is obvious." But I do put it to you, please think about that very carefully; because although we may think we understand the point, we do *not* actually operate our factories as though they were organic. We have some links: we have telephone links; we have management committees; we try to bring things together. But do we very often really regard the factory as a totality, as something that is indivisible? It is very rare indeed, and this is a very important idea and not one to be lightly cast aside as " obvious."

If you are going to treat the factory as an organism, you have to have sufficient evidence for doing so. You cannot measure every item in the situation. People who try to investigate an industrial process can see some of the major factors, but they are very well aware that what Bill Jones had for lunch is affecting him in the afternoon, and what Joe Smith's wife said to him the night before is having some effect, and so on. All these things so genuinely influence behaviour in the works. People realise this and feel instinctively that they will never get the model right.

The Black Box

This is a very real problem; but there is an answer, and the best way to put it to you is, I think, through a specifically cybernetic idea—the idea of the Black Box. Cybernetically speaking, we use the Black Box for a special situation which is worth getting hold of.

Imagine a Box with some wires going in and some wires coming out. These wires are connected inside in some way, but we do not know how. A number of states can be set up in this Box; that is, there exists a number of ways in which the connections can be arranged. This number soon becomes very large indeed. In the discussion on Grey Walter's Paper about cybernetics,* I gave details of a Black Box with merely eight inputs and one output. This has two-to-the-two-to-the-eight connections or possible states. This was printed in the *Journal* as 2^{28}, which is not quite the same thing. Two-to-the-two-to-the-eight is 2^{256}, which is a terrific number. I was anxious to get that across when I was referring to Eddington's Cosmic Number, which is close to 2^{256}. For this Number is said to give the total number of the fundamental particles in the whole universe. And yet you could label every single particle with the Black Box of just eight inputs and one output.

Why have I gone into all that? For this reason: that if you were trying to describe the working of that Box you could never do it by trying to arrive at all the possible states and listing them all and setting them all out. There are too many. That is exactly the situation you are up against in tackling an organic set-up in the works. You know you cannot define and set down and measure and combine all these things, all these factors.

Let us go back to cybernetics. You *can* describe the Black Box in terms of the way it behaves as a whole. This is the idea of the organic factory I was trying to put across. You have to have the model operating as the situation operates. Never mind about the individual bits, whether they are right or not. You have to have something that behaves overall in the same way as the overall situation.

Let me give you an example. If we tried to build a model of the national economy by putting in all the factors, we should be here for a very long time. Every facet of our life would have to be represented—probably the propensity to consume on the part of everyone of us in the whole nation would have to come into it.

* See January, 1957, Journal, pages 12/13.

But there are some things about the national economy that you can build into quite a simple model. You know what would happen in general terms if you raised the bank rate or increased purchase tax and so on. There is a model operating as an organic whole. You may say, "Very well. Why does not the Chancellor of the Exchequer do a better job?" I should be inclined to agree. Better econometric models are available to him than the "Bradshaw" of which he complained last year.

There is the idea. The fundamental term for describing the Black Box is the theory of probability. There is a lot of talk about the probability in horse racing and football, although the result is not always what people expect. Despite the colossal amount of knowledge and science that exists about this subject, the reason why the outcome of a gamble is unpredictable is obvious. The sort of thing that can be predicted is not the sort of thing that means money to people who would like to use probability theory. If that theory is no use the reason is that gambles are not usually done on things about which people with scientific knowledge can give an answer: it is this that *defines* a gamble.

Do not let all this blind you to the immense importance of this subject, which is very great indeed. It is quite phenomenal the amount of work in both logic and mathematical statistics which has been done on probability. But this is not a lecture on probability.

The Idea of Variation

The key is the idea of variation. This is the only thing we really need to understand. Operational Research *is about* that understanding. If I say to anyone here tonight: "Please understand that you have a process which varies. It is not always exactly the same," they will say: "We know that." And yet it is amazing how people do behave as though the process never varied. They get an average and they do things with the average; and it really is remarkable how they think to themselves, "My cost last month was 2.13d. My cost this month is 2.14d. Why have I got worse?" They have no regard to the variation implicit in the process and to the fact that this "change" is probably illusory and due to chance alone.

Probability theory in operational research brings in this idea of variation and an understanding of the nature of variation. It uses this idea in the way I have described in the Paper to give mathematical shape to the sort of things we find so hard to describe—the Black Boxes lying round our works in the field of production and sales and research and everything else.

Of course, the knowledge which contains all these techniques is statistics, which seems to many the devil of a subject. The whole point about it, the whole justification, really is this very point: that it helps to bridge the gap between theory and practice. Because *theory* is, on the whole, predicting the static average. *Practice* is throwing up all sorts of chance variations. O.R. describes these by mathematical statistics, sets of little probability curves. And that is the answer.

But these things have to be built into our management control. I feel this very strongly indeed. It is no good producing a series of reports about the way things act, because if you sit on the touch line and shout advice at the players you do not get very far. You do not get much done. But in any case if you sit on the touch line and try to describe the game you miss a lot. You have to be identified with the game.

Operational research has to participate in management, to take its place and its responsibility in managerial control. It is no use drifting in and saying, "Go in on that and if it goes wrong don't blame me." Recommendations must be built into the fabric of management. That is why I have expatiated at some length on the question of management controls, and about bringing in methods which will automatically filter information to the management so that it only looks at things *worth* looking at.

I feel I shall have a lot of sympathy at this point from management, who are inundated with figures and reports and information. We have sold ourselves the idea that information is vital and we must have it. But now we are getting so much that we have no chance to absorb it, nor to find out what it is telling us. One of the things we have to understand that operational research can do for us is to take these results, and do something with them that will deal with these chance variations and present management with the ideas that are important. A lot that has been done by our teams up and down the country, and a lot more that is going to be done, can be made standard practice. It can be done automatically, what is more; and we have to tie this up with our office colleagues, and build the data-processing machines that will be required, electronic filters which will verify important information, and so on. This may sound like science fiction, but it is quite possible and it ought to be done.

I could talk for a long time about the ideas of estimation and prediction which arise from probability theory, but I had better get on. I have said in the Paper that you cannot build in every separate factor scientifically. What you have to do is to find out how the *model operates*, and to tell management what will happen; not just like that, but what will happen *in a given set of circumstances*, supposing the model to be right. And it had better be right!

Next I want to discuss the question of complexity versus simplicity. It is important to understand why it is that our kind of work not only must be complex in appearance but had better be complex, or it will fail to do its job. The argument is simply that if the situation you are trying to describe and control is a complex one—and heaven knows it probably is—you must describe it adequately with a complex description. It is admittedly the task of the operational research man to reduce it subsequently to a solution that can be carried out, to put his results in the simplest manner possible, and to build his techniques, if possible, into working controls of a simple kind. But the work itself must be complicated.

People often say—I have often heard them—"Let us have simple controls, simple descriptions; then nothing can go wrong." That sounds true, but it is the wrong approach, because unless the model is, sufficiently complicated nothing can go *right*. Furthermore, nothing can go wrong if the model is really engineered to describe the situation.

I have tried to deal with this question of simplicity in the Paper and it may well be that this is a management problem. I think it is a problem of good management to try to solve this difficulty that the scientists find themselves in when presenting their ideas.

Then I have gone on to deduce some of the results, some of the conclusions reached in practising these techniques of measuring, and I talk about productivity. I do not know what you think about my views, but I believe in them very strongly. Thus, we could ascribe the whole question of low productivity to laziness and write it off; but we should be quite wrong. It is the wrong method again. There *is* laziness; but there are other reasons and I do not propose to go through them because they are complicated. I am perfectly certain, however, that the real trouble is in our design of controls. We have to get those controls right so that they lead to higher productivity.

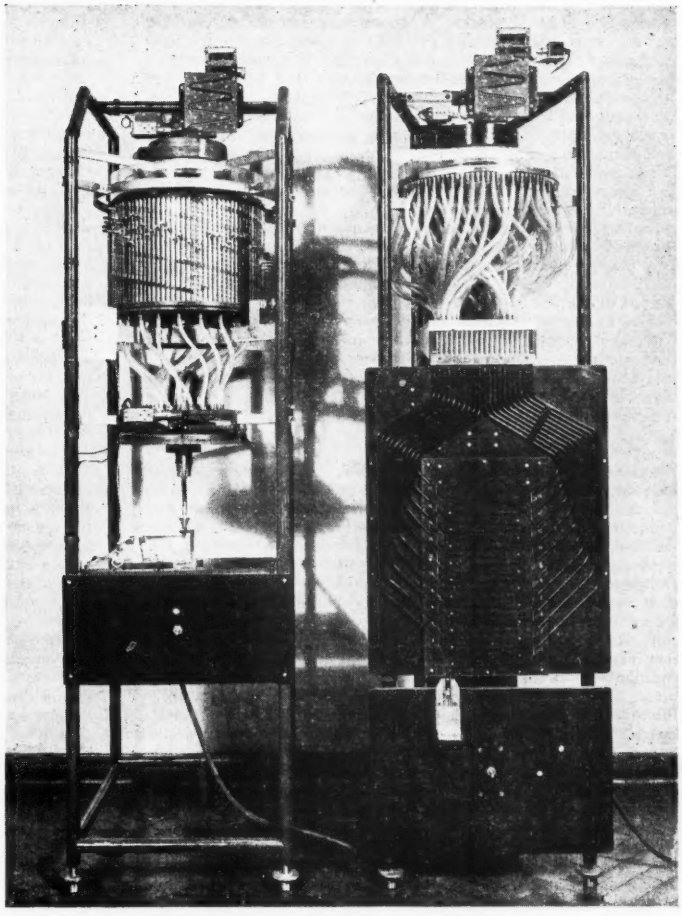
The Organic Works

That brings me back to the concept of the organic works; because that is really the thing you discover when you analyse the figures, when you analyse what is happening. And unless and until you treat the works as operating organically, as a whole Black Box, you will not get the right answers to questions like productivity. We all have a tremendous amount of reserve up our sleeves which can be got mainly from manipulating the structure of the operations we are doing.

I have finished the section on basic methods by asking for the integration of these techniques with the management methods themselves. I have mentioned that already tonight, but I want to end with a note on this section. We must not produce reports and just get them filed. We have to try to build our work into controls which constantly operate and work all the time. I do not know what you think of the little bit of symbolic logic I put in this Paper, but there is at least one man in the country—believe it or not—whose job is to sit in on works management meetings and analyse what is going on by symbolic logic and pull people up when they make a mistake. That is a very remarkable development, you will agree.

Coming to the question of problems and techniques, I hope I have given some idea of the basic operations we use with our models and probability measures. Let us come to detailed techniques and the problems we can solve.

I put first and foremost the question of stocks and flow and the simulation of situations surrounding stocks and flow. Above all things, I think, this is a major problem for us in this country. If you will look at Figure 2 of the Paper you will see the way in which I have tried to portray the nature of a stock situation. This is a simple idea, as you can see. It becomes complex



'S.A.M.'

The Stochastic Analogue Machine.

because whereas I have two factors there, you probably have to put in fifty; so that the model becomes a bit of a mess.

The idea is quite simply that there are two probability distributions. I hope you have followed the idea, and I need not labour it. The given measurable statistical risk for each of these process times will be more or less than the average, and that risk is delineated by the curve. We can define the curve after measuring the thing in practice, and we can draw the curve and describe it mathematically. Because of that we have means of deciding how these two things will interact in practice, and this is the main thought.

What is happening is that there is no connection between the two things; because a job takes longer on process A than you expected, it does not mean it will take more or less time on process B. (If it takes a long time on process A, and when it goes on to process B you have to rush to get the job out; or if conversely, a long process A spells trouble which must affect process B adversely, then there is correlation which affects the whole.) Other things being equal there is no connection, however, and you are up against two statistical risks which you can measure and understand. But this interaction is a matter of complete chance. How will this work out? We have to measure the queue which forms between the two processes. If a negative queue forms there is a production vacuum and it means there is low productivity. If the queue goes on growing, it means that you have to do something about it: there is a bottleneck. If you can describe this set-up in certain statistical ways, you can solve it by the theory of queues by sitting down with paper and pencil and doing some mathematics. But often the situation is too complex for this. That is where some form of Monte Carlo method comes in. The Monte Carlo method is one in which you simulate the situation through a model in a laboratory, as it were—at your desk by statistical techniques, and find out what happens. You are running an isomorphic model and finding out, getting an estimate of, what happens in reality. This is tedious to work out by hand and that is why we have produced this machine (see photograph above).

I do not propose to spend long on this machine now, because I realise that a lot of you cannot see it. But I would say a word about it because a fairly large section of you can see something.

This machine ought to be electronic in the sense that it would work more easily and quickly and probably more accurately if we made it electronic; and that is what we shall probably do. The reason for making this mechanical version is to try to explain it to people, to let them see what this process is doing. It is roughly doing this. Each of the ball bearings represents an item of some kind—an ingot, a billet, a ton, any item you like to name. It is going to be put through the Synthetic Data Generator, and it is coming out below. The interval between outputs represents the process time or some other time you have to simulate. The structure of the machine guarantees that we can reproduce in the long run any one of the probability patterns we like to write down. You can draw one, if you like, which is not real at all and simulate it. In the long run we shall produce frequencies which conform to the measured statistical risks; in the short run it is quite impossible to say what will happen next.

This is essentially a machine *designed* so that we do not know what it is going to do next. It is the principle of randomness which produces the next item in a chance order. That is exactly

what happens in the works. We know very well that in the long run our mathematical statistics will predict the shape of the kind of diagram I have given you in Figure 2. We shall get one of these humps of probability. We can predict that, but we cannot predict the order of items within it. We cannot say the next job we do is going to take eight hours. What we can say is there is a risk of running into eight hours, and the machine will produce this event proportionally to the risk.

In the machine we have synthetic data about the process. Here is a job coming off the machine (*demonstrated*) and another one going in. Now we start this one running (*demonstrated*). This is a differently designed machine but it amounts to the same thing and will generate the second of these probability distributions. It is generating in the same way. The answer which is coming out here is the output of the first machine and the input of the second machine. It measures their interaction.

You can *see* the interaction between the two machines. A ball has now gone in, and one will come out in a minute. This store is where we measure the rate of stock arrival and departure: it dynamically computes the queue. I do not suppose that is very clear, but I thought you would like to see it; if you are sufficiently interested you can look at it afterwards.

A National Problem

One of the points I want to make about this stock idea is that it is not a little local problem at all, but a national one. The stochastic flow problem is a very real national problem, not just a phenomenon we run into in our works, although it is serious enough there. What I want to get across is that we have a national stock, starting with primary industry, going down to secondary and tertiary industry, and down to the ultimate point of the system. Every one of these stockholding stages is represented by one of these probability curves, and the *risk* of one store running out in supplying the next all the way down the line is translated into the *stock* which that industry holds (or that company, or that department). The point is that in a time of boom, such as we had after the War, everything is in short supply. Buying policy throughout the country tends to be " If you see it, buy it. Let us have it." Stocks become bigger than they would be if they were worked out scientifically in this way. Then what happens? There is a recession, money becomes short, and people all down the line see trouble coming. They say: " Our stocks are too big. Let us cut them down."

We are so ignorant of one another's economics down the chain. We do not properly know what is going on, although we may suspect it. So there is a snowballing effect of panic, and that is how the trouble really starts. Everyone is familiar, I think, with that type of mechanism. It has happened in the past, but it should not happen any more because we now understand very much better what the mechanism is. The reason I have told this story is not just to bang a big drum, but to point out that you can only succeed with this type of control insofar as you have *information*. The reason we have trouble nationally is that we do not know enough about the other industries. They won't tell us. In our works we all know that if our departments have this information we can get it if we ask for it.

The Hon. Francis Balfour (Arthur Balfour & Co. Ltd.) *(centre)* chats with Mr. C. F. Hurst *(left)* of the Sheffield Section Committee, and Mr. F. T. Nurrish, M.B.E., Joint Managing Director, Geo. Bray & Co. Ltd., at the reception.

Now I am going to ask you to think of control problems not so much in terms of measuring and so forth as in terms of a balance, of holding a balance in a situation, of optimisation and linear programming. We come back to the organic works, the balanced organic works. All these techniques are required, of course, in an organic system if it is to be operated in safety. Equally, we need these homeostatic methods in the works and I have said a lot in the Paper about how it can be done. I have said a lot also about decisions and the way in which one can use logic and statistics to take decisions according to the principles about which I have been talking this evening; because otherwise you cannot. I point that out. You may have this approach and agree with it and feel this is the right line. But you have to have special techniques for taking decisions for the simple reason that there is a Black Box. And you are somewhere in it, sitting among the connections. You cannot really work out your decision because of the huge number of possible states surrounding you. You need a special technique for decision-taking within that framework. You have a number of methods, the last of which is the theory of games.

I wrote this Paper some time ago. I knew I was sticking my neck out when I spoke about industrial disputes and the handling of them by the theory of games. I did not know how far, in that I would be addressing you on the first day of what looks like being very nearly a general strike ! My neck has been stuck out for me, and I am prepared to accept that. I am not trying to pretend to you that we can go in with operational research techniques and solve all these difficulties—not for a minute. All I am saying is that it is high time someone got down to the task of finding out exactly how to do this scientifically.

May I remind you of these remarks which I am quite certain you have all seen in the last few days? " We have an unanswerable case. We are not going to be dragged through a court of enquiry or arbitration." That is one side. The other is: " We do not want Government interference. We want to fight it out ourselves." It seems to me a remarkable thing nowadays that we should be faced with such a sterile situation. If other methods have failed and people have found that they cannot talk about these things and put them right it is time some rigorous method had a go. There is a chance that with operational research you could do something about it. If that is not so, it is a pretty poor outlook for both management and labour. I am going to leave it at that, and I am prepared to endure any amount of laughter: because I am on pretty thin ice, I know. I hope no one will ask me to resolve this strike. But surely we ought to follow up any clue that will give us a logical and scientific approach to this problem, in the middle of the twentieth century when we ought to have grown far beyond this sort of behaviour.

I will leave that before I become too excited, but I feel very strongly about it.

Industrial Cybernetics

In conclusion, I must say something about the subject of cybernetics. I must admit that I would rather have been talking about cybernetics than about operational research; because cybernetics is a science in itself, a science in its own right. It is not just a set of techniques; it is probably going to become of simply colossal importance. But in order to understand the first thing about cybernetics, particularly industrial cybernetics, as it will come to be called, I think (though one would wish for a better name), we have first to understand operational research techniques. I do not think you can do it without understanding the sort of thing I have been talking about, and that is why I have left cybernetics to the end.

I do not say much in the Paper, either, because I felt I should be going outside my terms of reference; but there are three types of approach which ought to be known.

First we have the Black Box that is here in this Paper. We are using it as a model of the living organism. We are using it for operational research, and a lot of people have made very good use of this already. It is as though one were applying physiology to a complicated servo-mechanism where one is up against exactly the problems the body would be up against: the balance, and the recognition of certain limits in certain directions.

That is the first thing we are doing: it is cybernetics already with us.

The second is that, insofar as we can get facts, we can use cybernetic techniques to examine social problems; particularly

those we are creating ourselves, such as automation and so on. That is another field.

The third field of cybernetics is that of the so-called thinking machines. Very seriously, be very wary before you are drawn into saying you think these things are absurd and cannot exist. Because they do exist. It is just that these ideas are so new that they have not been brought out yet. I am very happy to tell you that we have two leading cybernetic people here, both of whom have produced machines of a remarkable kind—Dr. Ross Ashby and Mr. Gordon Pask. Dr. Ross Ashby is responsible for a series of machines known as homeostats, and Mr. Pask has built a series of machines which he calls teaching machines. I do not know whether he may be persuaded to tell us something about these, but they are there. A man would be rather brave to say they do not exist.

This machine (S.A.M.) has itself some cybernetic status. It is not a balancing machine, nor a goal-seeking machine, nor a teaching machine; it is in a sense an " experiencing " machine. Its fundamental cybernetic characteristic is that it is not determinate.

There is really, I think, too much talk about the problems of industry, industrial relations problems, productivity and so on. I have tried to put into this Paper some cogent and useful techniques, things which *work*, things which really can be done, which have been done in my Company. I feel operational research and cybernetics are not unrealistic and remote studies, however they may appear in this hall. They are an exciting new dimension for management to explore. It comes back, as it so often does, to this: let us *think the thing out*, and not just talk about it, still less rush in and do it. As President Eisenhower said to Mr. Dulles, " Don't just do something, John. *Stand* there!"

The President said that Mr. Beer's interpretation of his Paper had been both interesting and enlightening, and he strongly recommended everyone to read carefully the full written Paper.

There must be in the future a more scientific approach to our problems and in the years to come those associated with any applied activity would not only need to know WHAT one was doing but WHY it was being done.

He advised that even though one might not fully understand the Paper, nevertheless it would be good exercise to try to understand the important principles involved.

The President then referred to one aspect of operational research relative to the factor of " surprise ", and he made an amusing reference to his own experience during the First World War on airships of the Royal Naval Air Service, where the element of " surprise " was part of the day's activity.

With regard to the application of this subject by management, he might interpose here that the lecturer rather inferred that he was talking about works management. There tended to be too much talk about works management: one had to be very careful when talking about management. There was Government management and that could be good or bad. There was trade union management and that too could be good or bad. Works management again could be good or bad. All good management theories and studies should be understood and applied by *all* and not just in a factory.

It would also be wise to follow the lecturer's advice to be " in the game ". He liked that because having been one of the players in industry for many years, he was sick and tired — as he expected were most of his colleagues — of getting advice as to what ought to be done. There were 52 million people in this island. There were about 22 million gainfully employed and there were 9 million in industry! Therefore, he commended the lecturer's statement that one must be " in the game " to understand and not just talk theories.

With regard to the " black box ", to which the lecturer had made many references, he gathered that they had been talking about the " black box " in their own factories.

He had given a lecture recently on " People in Industry—The Changing Pattern of Industrial Society " and in order to do this he had made some close study of the subject. It was interesting to see how completely interrelated were the units of industry in this country. There were small groups, medium-sized groups and large groups ; but 73% of the factories in this country employed less than one hundred people, all integrated and interrelated. There were all these different sizes and types of " black boxes " about the country and, therefore, the only thing that one could predict apparently was the *inability* to predict accurately in every case.

It was important, however, to endeavour to understand the meaning of some of these new words and definitions, particularly for the younger men who were making, or should be making, a study of the more scientific aspects of operational research and cybernetics.

Mr. John Harling (*Orbit* (*Operational Research*) *Limited*) said Mr. Beer's Paper was an extremely lucid and cogent summary of the basic philosophy and techniques of operational research.

As a practitioner in industrial operational research for several years, perhaps he might usefully fill in, very briefly, with a few notes on the practical problems in industry that had been tackled by operational research both in the United States and in this country. He did not want to detract in any way from what Mr. Beer had said: Mr. Beer's brief was to say what operational research was, not what practical problems it had taken. What he himself had to say now, therefore, was in the nature of a small coda to an admirable Paper.

Cases which frequently arose were those involving inventory control or stock control. Two cases with which he had had something to do came to mind. One occurred in the United States where a manufacturer of synthetic detergents wanted to know what his run length of each detergent ought to be and how much stock he ought to hold. Currently, he was producing at a level rate to meet a level non-seasonal forecast. As a result, he had nothing in the way of production change-over costs; but part of the time he was heavily overstocked and part of the time he was completely out of stock. The operational research team was able to find the best balance between changes in production and the level of the inventory. Obviously, every unit in the inventory cost so much to carry for a year. On the other hand, every change in the production schedule also increased the cost. What was the best cost balance between the two? A simple operational research approach was able to give the most economic answer to that question.

Another useful case of stock control occurred in a raw materials warehouse, where there were deliveries with uncertain delivery times from manufacturers, and appropriations from the warehouse to the production schedule. In this case, it was found that both the manufacturers' delivery times and the size of the production appropriations each week did follow well-known theoretical probability distributions—the kind of thing Mr. Beer had described with the bell-shaped curve in his Paper. Accordingly, the managing director could be told that if he had so many dozens in stock that would represent a certain level of protection against running out of stock, just as though he were buying insurance.

Had it not been possible to make this investigation with apparently irrelevant and abstract mathematical models, it would have been necessary to leave the warehouse piled high with certain items and chronically out of stock of others.

The last example was what Mr. Beer had called a simulation which had involved the use of an electronic digital computer. The purpose of the simulation had been to imitate the behaviour of a port loading and unloading crude oil and petroleum products. The computer was able to allow the vessels to arrive, state the hour at which they were arriving, berth them if there was a berth or else put them into a queue. It could declare how long they spent being serviced and could add up the total amount of waiting time per year. It was therefore possible to relate the total amount of ship waiting time, and hence waiting costs, to the various arrival densities and servicing time lengths.

That was really all he wanted to say. He wanted to fill in a little practical background. Certainly Mr. Beer and he and a lot of other people in this country did feel that operational research had a profound and practical contribution to make to the profitability of British industry.

Mr. Allan Ormerod, A.M.I.Prod.E. (*Ashton Bros. & Co. Ltd., Hyde, Cheshire*), said he had come to the meeting with a fairly clear picture of what he intended to say. After listening to Mr. Beer's expansion of his Paper, however, he felt ready to disappear into the Black Box! It had been suggested that

operational research was the appropriate technique for solving problems as varied as military strategy and industrial disputes. He found that the questions he wished to ask and the observations he wished to make were pitched at a somewhat lower level.

As Production Manager of a single organic unit employing some 3,000 operatives, he supposed his factory would be regarded as of large to medium size. This appeared to be a reasonable assumption, following the President's statement that 73% of the workers in British industry were employed in factories employing less than 100 workers.

It would be quite uneconomic, in a unit of 3,000 operatives. to carry an O.R. staff of so many disciplines and so highly specialised as that outlined in the Paper. How was this type of operational research technique to be applied to the bulk of British industry? How could it be organised so as to produce the maximum results in the shortest possible time? The lecturer was employed by a very large company and it seemed credible that cybernetics might be useful techniques under these special circumstances, but Mr. Ormerod was still not certain that they could be economically justified in small-scale businesses.

In addition to this general observation, he had one or two detailed points to make. One related to the isomorphic model. Admittedly, Mr. Beer had said the example quoted was a simple one; indeed, it was a very simple one. He himself had tried to develop isomorphic models for the output of certain textile machines where every variable in the mathematical model was itself the subject of a frequency curve. He did not think the point was made in the Paper that industries using *natural* raw materials of great inherent variability were considerably more difficult to study in this way than those using *isotropic* materials such as steel.

On managerial control, the lecturer had pointed out that managers were becoming inundated by paper or paralysed by the system. He said that the manager frequently had no time to " leave his desk to look and think." This seemed to be implicit reasoning. If the manager spent his time looking, there was no necessity to wade through masses of data to see what he would have seen had he been there!

He wanted to make this point with considerable force, because industry was tending to become inundated by statistics and figures; and the Works Manager was frequently in something of a dilemma. He was faced with the very real problem of determining how much time to spend on the shop floor and how much time to spend looking at data. There was clearly the possibility that a future generation of Works Managers would come to regard statistics and figures as the inebriate regarded a lamp-post—more for support than illumination.

In the section dealing with productivity, there were some figures to which he would like to draw attention. It was suggested that if the machine operator increased his output " to sweated labour rates," he could only produce " two to three per cent." more. Whilst he was not familiar with detailed operations of the heavy steel industry, this was certainly inconsistent with general experience in light engineering and textiles. This atypical result was probably due to the fact that heavy steel operations were machine-paced, while light engineering and textile operations were not. A more appropriate figure would probably be 20 to 30% for a cross-section of British industry. This latter figure was also accepted by production engineers in Germany and U.S.A.

There was one final point he would like to make on the isomorphous model. The mathematics of course is never wrong. Where the theory sometimes goes wrong is that the model is far from isomorphous. He could illustrate this best by a short story. During the War he had the honour to serve a most distinguished engineer of international reputation. On one occasion, he was expounding a particularly involved hypothesis, and his chief drew his attention to a small card on the wall behind his desk. It read somewhat as follows: " According to the latest aerodynamic research, the bumble-bee cannot fly because the aspect-ratio of its wings is unfavourably related to the mass of its body." The caption continued—" but the bumble-bee doesn't know this, and keeps on flying just the same."

There were many bumble-bees awaiting those who would make isomorphic models of industrial situations. Machine interference problems where the operator did both inside and outside cycle work was just one example.

Mr. Beer, in reply, noted that Mr. Ormerod was castigating him for talking about industry on a big scale—and yet had asked him to say how maximum results could be obtained in the minimum time for the whole of industry. There was only one answer—to go in for operational research on a wider scale. And this was not happening yet. There was not a major industry (except the one mentioned at the end of the Paper) which, as far as he knew, was without *some* operational research. There was hesitancy about it still, although it was bound to come. The size of the firm had nothing to do with it. He himself had operated O.R. in factories with 7,000 employees, and down to 200 or 300, without any worry.

Mr. Ormerod had implied that each variable was itself subject to variability. He himself entirely agreed, and that was why there was a statistical approach.

Mr. Ormerod had said that his own industry was more difficult than others; and this proposition had the great merit of being *universally true*! But he himself hoped it was not being rude to the audience to say that steel was a lot more complex than people not in steel thought. As far as textiles were concerned, there was a very well-known exponent of operational research present (Mr. Swan) who operated in this industry, and perhaps he would say something later on.

On paper work, he thought Mr. Ormerod was making his own point, though perhaps he did not think he was. He said management was inundated with paper and wanted to go out into the works. That was what he himself had said. Mr. Ormerod had said if he were out there in the works he would not need the paper. What he himself had said was that one had to make sure the information produced to management was important; that it only got the facts that it needed to have. Those would be the facts about the plant, the mill, away down the road; the plant Mr. Ormerod was not looking at when he was looking the other way.

He had expected some people to disagree with him about productivity and perhaps he ought to explain the low figure a little. It was true of his own industry, steel, and they had not only heavy steel but a lot of engineering processes. He was well aware that the effort a man put in might be less than 97%. But take a man who was doing virtually nothing. If he suddenly began to work very hard, there was not a corresponding increase in productivity, because the contextual conditions were not right. This did not mean that there was no more scope for improvement in effort, but one did not get the benefits until one considered the situation as an organic whole.

The bumble-bee story was very nice and he thanked Mr. Ormerod for it. It did very well illustrate the difference between theory and practice, which—after all—was the subject to which they were addressing themselves.

The President observed that one most important way in which to encourage an individual to work to the maximum of efficiency was to make him aware of his purpose, both to the community and to life in general.

A typical example of this was at the recent disastrous fire at Jaguar's, Coventry, where staff and workpeople alike worked with such enthusiasm and effort that the factory was running again in an incredibly short time — they were aware of their purpose and the importance thereof, and this, to his mind, was an overriding factor whatever their knowledge relative to operational research.

Mr. H. Stafford, A.M.I.Prod.E. (*Chairman, Institution's Standards Committee*) said he started reading the Paper with an open mind tinged with scepticism. But the speaker and he were colleagues in the same organisation and he was, therefore, in the position of the husband who was asked to comment on his wife's new hat. He must consider domestic harmony.

Technically the Paper could not be criticised, but there were one or two points which Mr. Beer might answer. He placed great emphasis on unity of thought. One might venture to suggest, however, that what was really required was continuity of thought, which was more important. The idea seemed to be that the operational research team would consist of a number of specialists, each an expert in his own field. But it was first of all necessary to define the problem and after the team had done its worst or its best to apply the results. There seemed to be a need for someone to integrate these specialist interests. He had recently seen the results of an operational research that was completely useless, simply because the problem had not been properly defined in the first place. For success there must be continuity of thought and effective integration.

Mr. S. R. Howes *(second from right)* **discusses a serious point with Dr. J. M. Whittaker** *(extreme right)*, **Vice-Chancellor of Sheffield University. Others in the group are** *(from left)* **Mr. W. A. Hannaby, Chairman, Halifax Section; Mr. J. R. Widdowson; and Brigadier A. Levesley, Director, Edgar Allen & Co. Ltd.**

Mr. Beer had referred to the need for the education of the operational research man so that he made minor decisions himself before sending results to management. To quote the Paper, how was he to know whether Mr. Jones was telling him the truth unless he had practical experience of the problem or process or knew something about Mr. Jones' reputation for veracity? The operational research man must know this before he could make even a minor decision. How long would the education take and who would be the teacher?

The whole thing was bound up with practical experience. Mr. Stafford certainly agreed with Mr. Beer's theme about the utilisation of existing resources, since he knew from experience with standards investigations that one pound saved was actually more valuable than one pound earned; there was no tax to pay on it.

Leaving out cybernetics, all the processes and methods described in the Paper were already known and used and had been known for many years. What was new was the idea of integrating them and making one complete service to management. From his own experience with standards he could certainly agree on the element of surprise. In nearly every case where a standards investigation was carried out, there was some surprising element that arose out of the spotlight of investigation and had nothing whatever to do with the standard.

He had come across an example of a study on stock control not very long ago. A stock control classification and coding showed in this particular firm that they had 2,043 years of process stock at their normal production rate.

He found from collecting data on standards, which was very closely allied to operational research, that every standards investigation was a small piece of operational research. Collecting data and selling the idea were the main features and for the latter —selling the idea—personal contact was still the most satisfactory method. He would suggest that Mr. Beer added to his team a first-class public relations officer, who would not only be an operational research enthusiast but also acceptable to everybody concerned. To quote Professor R. O. Kapp " what is the good of being absolutely right, if you are the only one who knows it?"

Mr. Beer, in reply, thanked Mr. Stafford for his comments which—he said—were very much in line with his own thesis. The idea of continuity of thought was valuable; and it would be appreciated that he was trying to get at this in speaking about the integration of management. He agreed with all the final points, even the P.R.O., about whom he thought something had been done already.

The only question remaining to be answered as a question was how the operational research man was to know whether Mr. Jones was telling the truth. He agreed that a certain amount of experience was required, and a certain amount of education of Mr. Jones. But in case there was a Mr. Jones in the audience—which he was sure there was not—he had better know that a lot of statements and figures could easily be viewed with suspicion by statistical methods if they had been falsified. One had to be a statistician to fake figures in such a way that a statistician would not recognise them. They could draw their own deductions from that!

Somebody had to ask whether Mr. Jones was telling the truth or not. The man who asked the question was the only man who had any hope of answering it. The manager who was remote did not even know that Mr. Jones had been asked any questions. That was why the operational research man must accept some responsibility.

The President observed that it was easy enough to detect a blatant liar, but what was difficult was to detect an untruth being spoken by a man who genuinely believed he was speaking the truth. If operational research would help management, as it were, to cross-check statements so as to arrive at the genuine facts of the case, this would be helpful.

Mr. A. W. Swan (*Operational Research Dept., Courtauld's Limited, Coventry*) congratulated Mr. Beer on his Paper and said he had no criticisms at all, which would surprise the author as they had frequently had arguments. Like Mr. Harling, however, he wanted to add one or two points that might have been included.

He felt sure Mr. Beer would agree that there was this question of psychology in relationships with the people one dealt with; in fact, the salesmanship to which Mr. Stafford had referred. One had to keep in touch with the managers and the departmental people. When he himself, for instance, took on people he was also concerned, apart from such technical qualifications as he was seeking, with their personality. He was concerned with how they usually behaved and how they got on with people. This was just as important as the techniques. Indeed, as far as he was concerned it was more important.

Perhaps he might reply to some extent, on Mr. Beer's behalf to Mr. Ormerod; because he himself was in another branch of the textile industry. His department was doing a lot of jobs in a mill which employed 600 odd people and some of these jobs would certainly be of interest to Mr. Ormerod, whom he knew very well. If he would like to write to him (Mr. Swan) later on, he would be very glad to tell him what was being done.

One job they were doing was, for instance, to attempt to rationalise the twists in coning and that was quite a big job. They did apply operational research to a number of mills in their large organisation which consisted of units, and jobs were done in Nuneaton, Wolverhampton or wherever it might be. The medium-sized plant could certainly use these methods and as he had said, he would be happy to help Mr. Ormerod privately about what was being done.

One must not be blinded by the fact that the lecturer belonged to a big organisation. No doubt all sorts of little things had been done as well. They were not all massive jobs he had been talking about. In this kind of work, all sorts of individual bread-and-butter jobs cropped up and they were the ones to tackle.

Mr. G. Norton (*Orbit* (*Operational Research*) *Ltd.*) said he was surprised Mr. Beer had omitted from his list of references one of the best operational research papers ever written. It should be of great interest to members of the Institution of Production Engineers. It was " On the Art of Cutting Metals " and was by

F. W. Taylor. It was published in 1906, and it had all the ingredients of modern operational research. Perhaps it might serve as an important link between Mr. Taylor, whose name must be familiar to everyone in the hall, and a subject that was in danger of sounding rather difficult.

Although he was neither a scientist nor a mathematician, he led in a small way, very much from behind, an operational research team. He could do that because he was determined not to find it difficult. And he did not find it difficult not to find it difficult.

Mr. Beer, in reply, said he agreed with the speaker, and would add a little story. Many Sheffield people would remember a visit from Dr. Lillian Gilbreth a year or two ago. After the meeting he had asked where she stood in the matter of operational research, and she said that as far as she could tell this sort of technique was entirely modern, but was nevertheless the latest manifestation of the sort of work she and her husband had always been trying to do.

Dr. Ross Ashby said he was not in industry, but had come to be interested in operational research because of his work on the brain, trying to find out how it worked. The brain had been developed in evolution, had been through a very hard school of experience, and had developed certain basic methods or strategies. They were now beginning to discover these strategies, and they proved to be strikingly like those used in operational research.

The application of science to industry had been mentioned several times during the evening, and some speakers had suggested that as operational research was a form of science, and as there was a " science " that " proved " that the bumble-bee cannot fly, therefore operational research was probably useless. Here they must not confuse two rather different types of science. Until about three hundred years ago there was no distinctive science: there was only common sense and experience. Then Newton and Dalton discovered certain extraordinary *simplicities* that ruled over heaven and earth. Since then science had had two hundred years of triumphs, but they were apt to forget that the triumphs were gained by exploiting the *simplicities* that could be found in Nature. Whenever simplicity could be found, advances were sure to follow. And when simplicity could not be found, science tended to turn in some other direction, or to go on with attempts to find an underlying simplicity. When no underlying simplicity could be found, classical science tended to be baffled.

At last, however, they were getting to know how to handle the system that was *intrinsically* complex, and the new knowledge was collected in the methods of " operational research." It was certainly scientific, in that it dealt with hard demonstrable facts, but it was not " scientific " in the narrow sense of using those methods that had been developed specifically for the simple system that was neatly isolatable. It differed from what might be called " classical " science in several major ways.

First, its aim was not to get knowledge, but to get results. And the result wanted was always something practical and definite. Suppose, for instance, that a chemical process threw up a horrible sludge that was difficult to get rid of, and suppose someone discovered a treatment that got rid of the sludge without his knowing why. To the academic scientist, this outcome was not satisfactory, for he could not explain it. To the operational researcher, it was a triumph, for the practical goal has been achieved.

Again, operational research typically did not collect truth for truth's sake, but collected it only so far as it was relevant to the job. Having found an answer to a problem, it did not pretend that this knowledge had been achieved for all time, but only that it would endure until it become obsolete.

In operational research, the balance of aim and method was so different from those used in classical science that one could almost say that operational research was a different sort of science. It could never make the mistake of " proving " that the bumble-bee cannot fly, for the operational researcher did not think or work that way. He was much more likely to *use* the bumble-bee's flight to suggest modifications in the structure of small aircraft.

Those who opposed operational research on the grounds that science was not a reliable guide in the workshop were misunderstanding its nature. It was *not* the science of the past: it was a new science, that of how to get what you want when things get really difficult. There must be few indeed who could afford to ignore such a help!

The President said he was glad to see so many ladies present and felt quite sure that well-run kitchens and households were very familiar with operational research and cybernetics, but probably these were not the words used to describe a well-regulated household.

Dr. G. H. Jowett (*University of Sheffield*) said that so much reference had been made to the suggestion that these techniques were difficult that it might be worth while to mention that simple Monte Carlo processes and a number of other techniques mentioned by Mr. Beer were taught as part of the first-year course at the University. Once they reconciled their minds to them, students not only did not find them difficult but found them fun. They ran simple Monte Carlo processes with apparatus no more complicated than the little rolls of paper on memo pads, and they studied the buffering problem concerning stocks. Once they had spent an afternoon on it, they understood the basic principles perfectly.

Mr. J. L. Burbidge A.M.I.Prod.E. (*David Brown Industries Ltd.*) said he would like to add his congratulations to Mr. Beer on his truly inspiring Paper, and to acknowledge that he had completely convinced him of the importance to industry of both operational research and cybernetics.

There was, however, one point on which he would like Mr. Beer's further comments. In the section dealing with sub-optimisation, Mr. Beer stated:

> " There is a linear programming theorem which shows that this suboptimisation technique reaches the same answer whether the approach be made through higher profits or lower costs."

He did not believe that this theorem was true.

One method of suboptimisation already applied in industry was the use of a mathematical " model," such as Camp's formula, to determine the optimum size of batch for minimum costs in batch production.

The cost curve represented by this formula was one which in lay terms might be described as " rapidly falling " at the low batch size and as " relatively flat " over most of the values at the other end of the scale. (See diagram on page 331.)

Somewhere along this relatively flat portion of the curve there was a " minimum," which was known as the " economic batch quantity," or the batch size for minimum cost.

There was, however, another important factor which was also varied by batch size, and that was the average inventory, or in other words, the " outlay " of working capital invested in production. This tended to fall regularly with a reduction in batch size.

It was possible, therefore, to find many cases in industry where, if one started with the calculated economic batch quantity and then halved it, one found one had almost halved the outlay of working capital, but only slightly reduced profit margins. One had therefore increased the profits ratio and at the same time increased the cost per piece.

Not only did the profit ratio increase, but the total profit also tended to increase. As the economic batch quantity was on the flat portion of the curve, it was quite common to find when it was used that the capital invested in the second half of the stock was earning a marginal profit of less than 1%. It was difficult to find a less profitable investment for the money that could be saved by reducing the inventory.

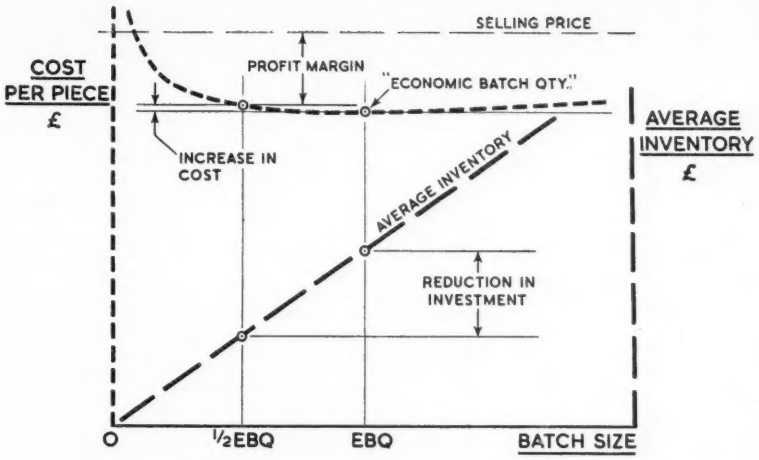
Exactly the same result was very often achieved, as had been found in Mr. Burbidge's own works, by replanning work from an ideal, but heavily loaded, machine on to more costly but immediately available capacity. The increase in turnover rate reduced the average inventory, increased profits and at the same time increased cost per piece.

He would suggest, therefore, that the theorem should be restated as follows:

> " Suboptimisation for minimum cost is generally incompatible with maximum profits."

He had been prompted to raise this point because he believed that this theorem, that minimum cost gave maximum profit, coupled with the concept of an economic batch quantity, was chiefly responsible for the very low rates of capital turnover found in industry and for an enormous loss in profits every year.

Like the example of the false theorem that " the world is flat," he believed the theorem that minimum cost was the same thing



EFFECT OF CHANGING BATCH SIZE – EBQ TO ½EBQ

as maximum profit, seemingly true, was nevertheless totally untrue. It had bedevilled industrial economics for many years and he suggested that it should not now be built into the foundations of operational research.

In conclusion, Mr. Burbridge said how much he admired the Paper and in criticising this theorem, which was borrowed from another science, he did not wish in any way to criticise Mr. Beer's own treatise.

Mr. Beer, in reply, said that this was a very interesting contribution and in the main he agreed with it.

The idea of the economic batch quantity was vital, and he too had had dealings with it. He agreed absolutely that the overall position could be improved in the way suggested, and the general thesis illustrated very well the importance of making an organic approach instead of looking at one bit.

He admired Mr. Burbidge's courage, but thought he had gone wrong; because he had been forced, in coming to his conclusion, to say that something which was mathematically axiomatic was false. It was very courageous to go ahead and say that, but with respect Mr. Burbridge need not do so.

There were a lot of conventions about costing. In a linear programme, when one said one was going to minimise costs instead of maximising profits, these costs were computed in the programme *as a function of the optimum load on the plant.* That meant a very different cost system from the conventional one. If it were adopted—and he suggested it would be a good idea to adopt it—there would undoubtedly be colossal national losses, and this would be rather stimulating. This was a major way of getting to the optimum situation; if standard costs were fixed in relation to the optimal load, then in order to reach the manufacturing " break-even " point one would *have* to adopt the best plan.

He heartily agreed that in practice what one was presented with as costs very often would not give the maximum results, and this was one of the worrying factors. If one was using the linear programme approach, and getting standards in that way, the plan was guaranteed to be the optimum. What he was really saying in a nutshell was that the ideal costs were those resulting from the use of standards which would be incurred if the plant was run in the best possible way. This would give a standard at which to aim which would ultimately be identical with the highest profit linear programme. He thought that resolved Mr. Burbidge's difficulty, without disputing a mathematical axiom.

The President said that the most difficult question had rightly come at the end. The evening had been most stimulating and he would now ask Mr. Widdowson to propose a vote of thanks.

Mr. J. R. Widdowson, M.I.Prod.E. (*Sheffield Section Chairman*) said they had gone a very long way from Archimedes to the subject with which the lecturer had dealt with so ably—the bringing together of a number of techniques as an aid to management.

After reading the Paper, he had gathered that most of the problems could be resolved into a mathematical model once all the information had been gathered together and analysed. He hoped this did not mean that they would all have to start studying symbolic logic to find the truth. But he did commend engineers and management to look into these techniques. The interest shown in the lecture must be most gratifying to Mr. Beer.

It only remained for him to ask the audience to pass a hearty vote of thanks to the author in appreciation of the able manner in which he had put over his lecture and answered the questions.

The vote of thanks was carried by acclamation.

Mr. Beer conveyed his thanks to the meeting which then terminated.

CORRESPONDENCE

Mr. G. Pask (*System Research Solartron Electronic Group Ltd.*) writes: I should like to take up a point made by Dr. Ashby. He has spoken of a distinction between " Use " and " Truth." Thus, in the past at any rate, science had been mostly concerned with conclusions which were " True " in almost the same way that " Two times two makes four " is " True "; just as " True," in other words, as the logical rules we choose to employ. Of course nobody pretends that we can have the absolute certainty about any scientific truth that we do have about a construct like a number. In the latter case we are certain just because we have agreed to follow these rules, whilst a gas in a chemical process has not agreed to obey the gas laws. However, it nearly always does so, according to our observations. When sometimes it does not do so, the gas laws can be modified for another specific set of conditions. The essential point is that *we can* specify these conditions, and under these conditions the " Rules " (the *model*) are applicable. It leads to *useful* conclusions, which are so generally useful as to approach a Logical Truth.

Mr. Beer, on the other hand, is dealing with a new field of work. In it there is no less precision than there is in science of the " True Conclusion " type. But the conclusions are less general—they apply only for a particular purpose or a particular system.

They are practical assertions, telling us, for example, how best to play if we represent an industrial system as a " game." The Rules (or *model*) which is employed has quite different characteristics to the Rules used before, as Mr. Beer has pointed out in his Paper. On the whole it is a much more general model, like a blank cheque which can be filled in to suit the circumstances. As a result it is often a much more powerful model. It may be worth examining it a little, and to lend emphasis let us select a system where *only* this sort of model *can* be applied. Thus Systems such as Mr. Harling has mentioned (the harbour system) can perfectly well be represented by a determinate model and their behaviour discerned on a digital computer. These systems *could* be dealt with according to *any* science . . . Select rather, a System which *does* look organic. Any imperfectly observed system can fall into this class. Maybe we cannot afford the time or money to observe it long enough to find the *appropriate model*. Perhaps, even, this model does not exist. In these cases the System *looks organic* and must be treated as such. The only tenable model, if we want to control the system and do anything about it, is a dynamic model of Mr. Beer's kind. Cases in point occur when we think about a *human population* or an *inanimate process* run under *critical* conditions. These, incidentally, are often the most *economic* conditions also.

It occurs that you may wish to do one or other of two things, but in general you cannot do both at once. You may wish to control the system (or the population) or you may wish to describe it. If you wish to control it you are concerned to have a simple relationship between the model of it and your control mechanism —which may, of course, be yourself giving commands and receiving data, or it may be a servomechanism. In the second case, of description, you obviously seek a simple model. This meeting is scarcely worried by description and in any case the descriptive techniques for systems that *look organic* are not fully developed.

Thus we want a simple " Model to Control " relationship. The price to be paid for this, the price of maintaining something like an isomorphism between the model and the matter of fact and at the same time having sensibly efficient control, is the price of not knowing the modus operandi. The model is " Difficult " rather than simple. It has what Mr. Beer has aptly termed a " Liveliness " about it.

But " Liveliness " is *not* " Indeterminacy " or vagueness. We have it simply because we want the system to be controllable and down to earth. Admittedly its presence implies that we cannot say in simple terms what model we are using (as we *can* say what happens in a chemical reaction). But the *reason* why we cannot is simply that we *must* and *can* say what we want—why we are making this observation—what the control is to achieve.

Very often a Control procedure is represented by analogy with a game, as described in the Paper. Here the kind of state to be achieved is described by the " Solution "—what we deem a " Best Solution." When a Determinate Model exists, The Solution may a von Neumann solution and the analogy is worked under the assumption that the " Game " is wholly competitive. The players, one of whom may represent the forces of nature, are, as it were, motivated by avarice alone. When, in the present case, there is no real model, beforehand, this analogy does not make sense; chiefly because we have no idea of what " Gain " or " Loss " implies to the " Other Player." The approach recommended by Mr. Beer is to use a Braithwaite type of solution, and a game analogy in which the implications of gain or loss are built up as the game is played. Notably this form of game yields a " Co-operator's Surplus." Its play is not entirely determinate because it mirrors a dynamic *lively model* representing the players, the liveliness implying co-operation. The Solution is, however, determinable in the sense that we can say *beforehand* that one or more features of a situation should be maximised. These, incidentally, can be anything: thus we might want to maximise " Yield of acetone," " Average Income," and " Incentive " all in the same system. It seems to me that this diversity of aim is one of the chief merits of the approach—and it is a most important merit. It goes at least some way towards showing how the Black (or Blue) boxes at different levels can interact together. It is possible because, whereas a Linear Programme is a game which works out the consequences of using a known model (like the harbour) this kind of game literally Builds the Model, given two things: (i) the purpose of the control system and (ii) interaction with the matter of fact. That is, the " Player "—control mechanism or man must be part of the controlled system. It would be interesting and valuable to have Mr. Beer's comments and in particular to know which of the several possible methods he prefers (or whether he has developed any special method) for dealing with games where many different attributes are to be maximised.

Mr. Beer in reply, writes:

It was unfortunate that Mr. Pask was not able to speak at the meeting, and I should like to take an opportunity now to reply both to him and to the other distinguished cybernetician present on the 18th March.

Dr. Ross Ashby was, I think, quite right to say that operational research is practising effectively a new *kind* of science. In his argument, he uses the word " truth "where I (as a philosopher) would prefer him to say " that which is empirically the case." In this way, we need not become involved in strange notions like " different kinds of truth," nor in pragmatism. I do not think that Dr. Ashby would argue about this: it is mainly a matter of words. Mr. Pask, however, does put the point very well. He says that O.R. conclusions " apply only for a particular purpose or a particular system," and that they are nevertheless perfectly precise—that is, within their own special logical language. A general statement to which I think all three of us might subscribe would be that O.R. is basically concerned (i) with the definition of a game situation, (ii) with the operational account of its strategies, (iii) with the recognition of feasible solutions, and (iv) with optimal pay-off for a simultaneous outcome in a given number of dimensions of utility.

If I were to allocate conventional terms to this account, I should call (i) facts, (ii) their context, (iii) truths, and (iv) the conclusion. But, of course, it is difficult to treat new thinking in language which was developed to deal with traditional thinking. Why I feel so indebted to Dr. Ashby and Mr. Pask is because they have so ably pinpointed the essential novelty of our work in O.R. and cybernetics. If we were to say, defensively, that there is nothing new here, we should be confusing concepts and techniques on the one hand with aims and objects on the other. Some of the contributors to the discussion have failed, in my submission, to make this distinction. Our *objects* accord well with those of Taylor and Gilbreth; but our outlook and method is utterly different. Nor should it be overlooked that most of our specific techniques are the product of this approach: all those described in Part II are very new and very characteristic of O.R. thinking. One speaker implied that all these special techniques had been in use long before O.R. united them. It is important to deny this; the techniques are only comprehensible within their operational context.

Mr. Pask seeks my views on " games where many different attributes are to be maximised," and he agrees that the Braithwaite method offers a useful approach. But perhaps its most important aspect is its demonstration that, although the original von Neumann seam in game theory has appeared to many to be " worked out," quite new developments are possible. Within the framework of classical inductive logic, it would have been impossible to ask for the maximisation of more than one attribute of a situation. In real life we require, on the contrary, a prudential policy which handles *all* the preferred attributes. In the von Neumann game, we reach a minimax result which is certainly prudential, but which is also rather negative. The need is for a treatment which selects a group of desiderata as it goes, and maximises the *group* pay-off. This is, I take it, the interaction of Black Boxes at different levels of which Mr. Pask speaks. The commensurable measure for this purpose in the Braithwaite approach is the variant Preference Scale, and the two levels of language become common in the " isorrhope," which is a parameter of constant relative advantage.

I do agree that this outlook is likely to help us in approaching non-competitive games. When Mr. Pask asks specifically whether I have developed any special method of my own for this kind of situation, he refers to my theory of stochastic duels. In this work, the commensurable measure is called " winning potential," and this synthetic attribute is defined so as to embrace a dynamic group of desiderata of the kind we have been discussing. A definitive grip of this set is retained *statistically* (through the central limit theorem); and the common language for the two levels of Black Box is the complementary probability of stochastic events, when distributions of winning potential are considered to have unit standard deviation.

This is, however, not the place to develop further the exceedingly technical arguments of cybernetic research. For the second time within a few months such ideas have been ventilated before the Institution. I can only hope that production engineers will appreciate the industrial potentialities of the science.

AUTOMATION—
VERBAL FICTION, PSYCHOLOGICAL REALITY

by THE RT. HON. THE EARL OF HALSBURY, F.R.I.C.,

President-Elect of the Institution ;

Managing Director, National Research Development Corporation.

'AUTOMATION' is not a technological term, though it started as the nickname for a technological problem of much narrower scope than its current use would lead one to suppose. Its use, more frequently its abuse, pertains to journalism and sales promotion. Journalists for the most part seem unaware that they have, to all intents and purposes, coined it themselves. When they ask technologists what it means, the question ought to be referred back to them.

There is in fact considerable confusion involved in this matter. Journalists seem to think that there is a new technological process, called automation, to be compared in its novelty with nuclear energy generation or antibiotic manufacture, and that this new technique will result in automatic unmanned factories, the installation of which will cause widespread unemployment. Thus journalists have misled not only one another and the public, but trade unionists as well—some of whom have reacted by stating what they would do about it. These statements have been written up by the daily press as if a real situation were involved, and some current events (such as the strike at the Standard Motor Co.) have been interpreted in terms of workers' resistance to automation. The public has thereby been still further misinformed. The situation is rather like the growth of a rumour; no one can subsequently give a satisfactory account of how large numbers came to believe a story having no foundation in fact. Press and unions have in fact been stoking one another up till both have come to believe that what they are talking about corresponds to reality. Needless to say, the more responsible members of each group do not believe this.

What is Meant by Automation

Various attempts have been made at defining automation in general terms. These all exclude one or another of the activities which, by popular consent, are now called 'automative'. Consequently they are unsatisfactory.

I have suggested elsewhere that the only way of bringing order out of this chaos is to freeze the use of this word so as to cover every context in which it is now employed, and to extend it no further. Failing this, it will come to mean whatever the user has in his mind when he uses it, and by meaning everything in general will end by meaning nothing in particular. If this proposal is accepted (and no one has so far disagreed with it), then we are left with four unrelated topics to discuss :

1. *Transfer Processing.* That is to say, linking fabrication operations, normally performed individually and in sequence, into a continuous process of an automatic character.
2. *Automatic Assembly.* This resembles transfer processing in definition, except that it relates to the assembly of many units rather than to the forming or fashioning of a single unit.
3. *Control Engineering.* This is concerned with the automatic control of self-regulating processes, and describes devices of which examples are the speed governors of steam engines, thermostats, autopilots and so on.
4. *Communication Engineering.* Only part of this subject is involved, namely that part which relates to data processing, accountancy and mathematical operations conducted with electronic digital computers, and the linkage of computers to physical or chemical processes.

These topics will be described in more detail below. For the present it will suffice to have enumerated them briefly, as above, in order to continue the process of definition and explanation for the removal of misunderstandings.

First of all, many processes employ more than one of the above techniques. Thus transfer processing involves a measure of automatic control, and automatic control may involve the use of a digital computer, parts of which may have been fabricated using methods of automatic assembly.

Secondly, it should be noted that the fourth item above includes the use of the word 'electronic' to describe a particular device, the computer. As automation is linked in the public mind with electronics, it ought to be explained that there is no

necessary connection between them. Transfer processing is in fact far more dependent upon pneumatics (the use of compressed air) than upon electronics. Automatic control employs both pneumatics and electronics when suitable opportunities for either present themselves. Communication engineering is, in its automative aspects, almost entirely electronic in character. Automatic assembly does not at present depend to any great extent on either type of technique.

Thirdly, from the sociological point of view, it is impossible to avoid spurious distinctions arising from the foregoing. Thus if a milling cutter and a grinder, regarded as unit machines in sequence, are coupled by a transfer machine, the new unit process resulting is 'automation'. If, however, the grinding machine is rendered superfluous by high speed milling, the result is not automation. We have in fact no generic term for it, except 'advanced machine shop practice.'

Examining these four processes in search of a common feature which could be made the basis of a general definition, it seems that they possess one rather slender link in common, namely that they reintegrate processes, hitherto broken down into sub-divided tasks, by rendering the whole continuous. 'Re-unification in continuity' thus presents itself as a theme which could be the basis of a general definition, but it is too general to be of much use and would result in including old processes which no one has, as yet, classed as automative.

So far, this description runs parallel with that given in the report on automation sponsored by the Department of Scientific and Industrial Research,[1] except that the report couples transfer processing and automatic assembly together, and thus reduces the topics classifiable as 'automation' to three in number. There are, I think, some advantages in keeping them separate. The report, however, goes a great deal further by defining 'complete automation' as appropriate to cover the case of a completely automatic or 'push-button factory,' the administration and works management being supposedly controlled by a computer. In fact, it goes a great deal too far if the definition implies that such a factory exists anywhere on earth. If one is to take cognisance of mere possibilities (in the sense that there is no law of nature excluding them) in framing definitions, then why not go to the limit and declare the factory self-repairing, thus eliminating the maintenance engineers as well as the works manager? One would then be well and truly embarked on the writing of science fiction!

One could dismiss public preoccupation with the subject by observing that 'automation', as here defined, is no more than an arbitrary selection of topics culled from the current phase of advance in a two-centuries-old field, that of mechanisation. Why coin a special word for an arbitrary selection, implying that something unprecedented is taking place? This attitude is certainly tempting. It is one that many if not most engineers would like to adopt. On the other hand, there is often a measure of unconscious wisdom in a public attitude, even if overlaid with misunderstanding, and it may be advisable to take cognisance of it.

Technical advances may be of two kinds: steady cumulative advances on broad fronts, and concentrated advances on narrow fronts—salients as it were thrust out into the technological unknown. The topics selected here as automative are not in fact chosen entirely at random. They are just those branches of technology where interesting salients are at present being pushed out. They are the subject-matter of many technical textbooks now being written, though the word automation is not found in them, To some extent these branches abut—are contiguous. They may later join up into a major strategic advance, or they may not; it is too early to say.

In so far as their implications may be more far-reaching than one can foresee, there is a case for advanced consideration of them, if that is the public wish. In fact, it *is* the public wish. One may therefore accept rather than reject the word automation, but help the public to use it in a rational way. Be it remembered always, however, that automation deals with advances, not with novelties, and that it is but a sub-species of the familiar species 'mechanisation'. No one of the four topics is new. Like the Bourgeois Gentilhomme who talked prose all his life without knowing it, many people have been subject to automation for many years and were content, until recently, to call it 'advancing productivity.'

The Technical Reality

Transfer Processing

Transfer processing is not generally concerned with the manufacture of capital goods, such as electric generators, or consumable goods, such as textiles. It applies more typically to the manufacture of consumer's durables — products such as automobiles, refrigerators, sewing machines, typewriters and so on. These commodities are assemblies of sub-assemblies which may in their turn consist of sub-assemblies. Ultimately we reach *parts*. A part is itself the product of a sequence of fabricating operations. Considering an automobile as typical, it is an assembly consisting of sub-assemblies such as the chassis, the engine, the transmission, the gear box, the body work, the upholstery and so on. The engine is itself made up of sub-assemblies — the engine proper, the carburettor, petrol pump, self-starter, etc. The engine proper is made of parts—the cylinder block, the crank shaft, connecting rods, pistons, valves, bearings, and other such units.

Each of these parts results from a sequence of machining operations. Taking the cylinder block as typical, the top and bottom must be milled or broached flat and parallel, the cylinders must be bored, valve guides drilled, seatings ground, various flat surfaces milled on the exterior of the engine to which auxiliary sub-assemblies will be bolted in due course, and a multiplicity of holes drilled, some of

1. *Automation*, Department of Scientific and Industrial Research, H.M.S.O., London, 1956.

which must be tapped so that bolts may be inserted in them. Each of these operations is carried out by a special-purpose machine designed to do that one operation and no other. Some of these machines perform multiple operations, such as boring six cylinders simultaneously. As the function of machines of this character is to remove material, the production engineer is concerned to keep the latter to a minimum by appropriate design of the material fed to the machine. The generic term for an input of this kind is a blank. It will generally take the form of a casting, a forging or a stamping — the result of some prior, relatively unprecise, operation of a large scale character.

With these preliminaries, consider the function of a machinist. His task is threefold: first, to feed the blank into the machine in correct register therewith, by forcing it to abut on some fixed framework called a jig; secondly, to supervise the performance of the machine; and thirdly, to remove the finished work, and to inspect its dimensional accuracy by means of a gauge, thereby ensuring that nothing but good work is passed forward to his successor in the sequence of machine operations.

If the blank is small, it may be possible to mechanise the input process by means of hopper or magazine feed, of which a familiar illustration can be furnished in terms of the belt or drum feed of bullets to a machine-gun. If the throughput is large, it may be possible to arrange that it starts in response to the completion of automatic feed, carries out its operations automatically, and switches off to discharge its output on completion of its task. In this way an operator may be able to keep an eye on several such machines, inspecting the output of each in turn, subject to a suitable sampling routine.

If the blank is large, such as a cylinder-block casting, this approach fails; though it should be possible, in principle at any rate, to make the output of one machine feed automatically into the input of the next. This is the essence of transfer machining, and the issue is not whether it can be done, but whether it can be made to pay. Cylinder blocks are large unwieldy objects; moving them about automatically and clamping them into register to within a thousandth of an inch entails a large heavy machine for the purpose. Moreover, in committing himself to a continuous sequence of operations of this character, the production engineer commits himself also to the strategic snag that the whole line of machines must stop if, for one reason or another, any one of them stops, breaks down or goes out of adjustment. Furthermore, the mechanisation of the feed must be accompanied by mechanisation of the inspection procedure, and arrangements for rejecting defective work. Failing this, expensive machining operations will be carried out on worthless products.

The capital cost of providing these complicated automatic operations in relation to the capital cost of the machine tools will be an important consideration in determining whether transfer pays or not; for the object of transfer is primarily to increase output, and not to eliminate labour, which is a secondary objective.

The first such transfer-line was installed at the works of the Morris Motor Company in 1923. It was technically sound but economically unsound. The capital costs of machine tools in relation to their rate of output was not high enough to make the transfer installation a paying proposition.

The second attempt at a transfer-line was made some 20 years later by Fords in Detroit and was economically successful, the relative capital costs involved having in the meanwhile altered in favour of the transfer process. The engineers responsible nicknamed their problem 'automation', and the name stuck. (Various proposals for a general definition of automation have to be rejected on the ground that they exclude what Fords did in Detroit; a general definition of a word which excludes its historical origin would be inappropriate).

There is nothing which renders machining operations more suitable for linkage by automatic transfer than other engineering operations. The body work of a car consists of parts called panels, and each of these is the product of a sequence of pressing operations carried out on sheet-metal blanks. Linkage of these by automatic sequences constitutes 'transfer-pressing,' and the totality of similar operations may be called 'transfer processing,' one of the four topics to which the name 'automation' may be given.

The terminal economics of all such processes — that is, the economics of carrying an idea through to its logical conclusion, 'going the whole hog' or whatever one may call it — is a rather sensitive function of throughput. A production engineer tends to tackle first the easy problems which cost least, together with the rewarding problems which pay a high dividend. Next he will tackle willy-nilly any subordinate problems whose solution is required to make a bigger or better success of the first group of solutions. There will remain, thirdly, a group of problems which are not of prime importance, and whose solution is neither particularly easy, nor notably remunerative. The cost of solving these is usually determined by the law of diminishing returns. At the point where they engage the production engineer's attention, he is liable to be distracted by something more important elsewhere. But this may be all to the good, for there are disadvantages in overdoing a process of this kind. It becomes inelastic as it becomes more complex, and the cost of hold-ups and breakdowns rises. In order that a whole line should not halt by reason of an adjustment required to one machine, it is necessary to arrange reserve stocks of work in progress at regular intervals down the line in such a way that the line can deliver into the reserve or buffer stock if there is a halt ahead of it, or take its input from the buffer stock if there is a halt behind it. It may be convenient to make these points identical with some process requiring manual operation, the mechanisation of which would be tricky and difficult.

Matching all these necessities and opportunities requires detailed ingenuity by production engineers. They are not engaged in an intellectual game called 'going the whole hog' or 'eliminating the last man'. They are concerned with minimising costs. As to

'eliminating the last man', suppose the manual operators have indeed been reduced to one and one alone. Replacing him would require a machine and the services of a maintenance engineer. A special purpose machine which replaced on balance less than one man would be of very marginal interest. The 'last man' would be liable to linger at his post for quite a while! This point is of some importance. Automation is found in patches here and there. No factory is homogeneously automated.

The Russians, it is true, claim to have a completely automatic factory. Study of the published descriptions of this factory, however, scarcely confirms this view. The factory produces a *part* only. In fact it produces pistons for automobile engines. It would be a mere department in some modern automobile plants. In designing this factory, the Russian production engineers have coupled the melting shop and foundry to the machine shop. I personally doubt the economic wisdom of this. A melting shop and foundry, on the one hand, and a machine shop, on the other, work to such intrinsically different rhythms that I would have been inclined to classify their coupling as of the third type mentioned above, that is to say, 'not technically easy nor very remunerative'. I may however be mistaken. Only very detailed study of costs, which are not available in this case, can demonstrate whether a given device is economic or not.

Automatic Assembly

Automatic assembly is not new. If one wants to be pedantic, one can regard a loom as a device for the automatic assembly of cloth. This takes us back a very long way into the past. Electric lamp bulbs and radio valves (and latterly television tubes) have been automatically assembled for a generation at least. Recently some significant advances have taken place in this field. Anyone who has looked inside a radio or television set will have seen a tangle of electrical interconnections running from part to part. Each of these is a wire, the two ends of which are soldered to contact points at which other wires are also soldered. If there are 100 interconnections, then 200 soldering operations are necessary (usually made by young women workers). These connections have not only to be made, but inspected. The cost of inspection may be a multiple of the cost of wiring.

The first stage in mechanising this assembly is to print the pattern of electrical interconnections on a light board made of insulating plastic material. There are various ways of doing this, but they all amount essentially to the use at some stage of a standard printing press. The electrical components have next to be fitted on to this board. A component is typically a little cylindrical cartridge from a quarter of an inch to an inch long and from a tenth to a quarter of an inch in diameter. From its extremities project axially two leads of tinned copper wires. These components are customarily mass-produced by the million in factories operated by sub-contractors to the electronics trade. Fixing them to the printed circuit board is effected by piercing the board with holes appropriately located, stapling the two leads through the holes, and bending the stapled ends round so that they abut on to the appropriate parts of the printed conducting pattern on the board.

The reader will be familiar with the stapling pliers which are used in offices to clip dockets of paper together. A machine to clip components on to a printed circuit board would be no different. The components would require to be fed to it rather as bullets are belt-fed to a machine-gun. This can in fact be achieved without difficulty or expense, using expendable belts of paper tape, for example. A sequence of these stapling devices, suitably registered at 'stations', accordingly staples a sequence of components on to a board fed past the stations in succession by a travelling conveyor which halts under each station while the stapling operation is carried out. On completion of the assembly in this way, it is lowered whole and entire into a dip-soldering bath and all the soldered connections are formed simultaneously.

The manifest success of this sort of device in the U.S.A. (it is quite new, and believed to be not yet completely operating in the U.K.) has aroused great interest, and produced fairly typical consequences. First, the component designers are redesigning their products so as to make them more adaptable to automatic assembly, and secondly, production engineers are asking themselves whether there may not be other problems which could be solved by a similar approach. In the U.S.A., experiments have been begun towards the automatic assembly of automobile engines. A wide range of relative difficulty seems to be involved. One can visualise without too great difficulty the automatic assembly of rigid products, such as engines. Floppy objects, such as body-panels, present more difficulty, and pliable objects, such as upholstery, a great deal more.

Control Engineering

Control devices are not new. When James Watt patented the centrifugal governor in connection with his steam engine, he was only adapting a governor that had been used for some time on water-wheels and in flour mills. The thermostat is a familiar device on electric water heaters and radiators of cars. It functions as a temperature regulator, turning a source of heat on when the temperature is below a pre-assigned figure, and off when it is above. An autopilot embodies the same idea. It applies rudder to a ship, or control surface action to an aeroplane, so as to turn it to the right when it is off its course to the left, and to the left when it is off its course to the right. All such devices tend to maintain some quantity — such as, a velocity, a flow, a temperature, a direction, a pressure, a liquid level — constant at a pre-assigned value irrespective of an input which may be fluctuating, or of an output load which may also be fluctuating. They consist essentially of the following components:

1. A 'setting' device which designates the value required to be kept constant.

2. A measuring device which generates a signal indicating the actual value in question.

3. A subtractive device which subtracts component 1 from component 2 and so generates an error signal.

4. An amplifying device which converts the error signal into a source of power.

5. A servo-mechanism which applies power to the controlled system in such a way as to cause an increase in the signal (component 2) when the error is negative, and a decrease when the error is positive. This mechanism may be linear (effect simply proportional to cause) or non-linear as in the case of on-off systems such as thermostats.

The point to note is that these systems are diabolically unstable, and 'hunt' or 'oscillate' about the desired performance instead of maintaining it constant. This is particularly true of non-linear systems. In fact, the cure is often worse than the disease, and early attempts to stabilise ships against rolling resulted in their turning turtle—owing to the designer's inability to stabilise the stabiliser.

Guided missiles have given a tremendous incentive to designers in the control field, and our understanding of the all-important but very difficult non-linear systems has increased noticeably in the last 15 years. Current progress in this field is associated with the design of simulators, sometimes called analogue computers (not to be confused with digital computers, see the next section), which are all-electric models of control systems which may themselves be pneumatic, hydraulic, aerodynamic, etc. Apart from aeronautical and marine uses (for example, the Denny-Brown ship-stabiliser), the most important field for control engineering appears to lie in the chemical industry, where continuous processes involving multiple feed-back loops may, if unstable, be highly dangerous owing to 'surging'.

Control engineering is, from the standpoint of its importance, in somewhat marked contrast to the other topics embraced by 'automation'. In all these fields the advances have been primarily practical and empirical. In control engineering the advances have been mainly theoretical. This has led to an interesting situation in which control engineering terminology has been found to have a wide field of application in quite unrelated directions, notably in the physiology of the nervous system and in economics and sociology. In so far as it avoids control engineering terms, the language of political controversy in economic matters is becoming increasingly archaic.

Communication Engineering — Computers

In expounding an unfamiliar subject, two types of difficulty may be encountered—conceptual difficulties and complexities. In expounding the *modus operandi* of a digital computer, the difficulty is of the second kind. There is no conceptual difficulty, but the reader cannot be expected to follow an explanation involving the interaction of parts when these are numbered in thousands.

The following general picture can however be attempted in terms of certain familiar devices : the Morse code, a clock, a gramophone record, a telephone, and the operation of dialling a telephone number to an automatic exchange. These devices may be used for the purpose of illustration as follows.

The Morse code is known to represent numbers and letters of the alphabet as combinations of dots and dashes. Let it be supposed that we are dealing with a modified code in which a dash is represented by *the absence of a dot*. In order to be able to discriminate between the absence of a dot and a pause between dots, we suppose further that the transmission of information in the code is coupled to the ticking of a clock. The absence of a dot is then equivalent to suppressing a 'tick'.

Suppose next we have a gramophone record, dictaphone, tape recorder, or some such device on which 'ticks' transmitted by a microphone can be recorded (written) or reproduced (read).

Turn next to the telephone. The reader will be familiar with the fact that a telephone can be used in two ways — either for sending a message, or for dialling a number. When dialling a number, one can hear the succession of 'clicks' or 'pops' which occur when the dial is released, and which are the means of activating a response at the automatic exchange, whereby a circuit is set up connecting the subscriber to his correspondent.

Lastly, conceive that the telephone is operating to a rhythm ultimately derived from a clock, and that it is transmitting coded letters or numbers stored on the gramophone record or 'store' in such a way that during alternate periods the device treats the information from the store first as a dialling signal and then as a message. There is no distinction in kind between dialling signal and message. Both consist of clock 'ticks' with some of the ticks suppressed according to the code in use. The difference consists entirely in how the circuitry of the machine treats them. As described here, it treats them as an alternate series : dial—message : dial message : dial—message.

The effect of such a design is that the machine will perform a programme of operations in accordance with the coded 'information' on the record or store. If the unit alternation dial—message results in an operation of the form 'contents of store A via route C to store B', and C is the accumulator of an electrical adding machine working in the appropriate code, the effect will be that some number stored at A (A is usually called its address) is added to the number in the accumulator and transferred to store B (address B). A, B and C are merely *numbers* suitably coded.

Those familiar with accountancy will recognise that this is what an accountant is continuously doing : taking a sum from one account, adding something to it, perhaps, and transferring it to some other account (which could be identical with the first). For example, a bank balance might be extracted,

credited with the sum on a pay-in slip or debited with the amount of a cheque, and the balance thus modified written back into its original position.

The importance of this accountancy lies in the simple fact that, these devices being all electronic, the clock 'tick' which controls the basic rhythm of the machine can proceed at the rate of a million ticks per second. This is high-speed accountancy with a vengeance!

It is much more than that, however, for so spectacular an increase in speed opens up whole new territories for exploration, inasmuch as commercial or managerial calculations too laborious to be undertaken otherwise, become possible for the first time: calculation of optimum traffic routing, for example.

Alternatively, these machines open up a new technique in the field of management accountancy, so called 'management by exception'. In this instance, management only receives in the first place such information as appears statistically significant and requiring action. The computing device can select this information from the total information in its store according to criteria laid down for it in advance by statisticians and mathematicians.

Lest it be imagined that this would lead to a managerial rigidity confined to a framework of rules laid down in advance, it should be added that with machines of this kind management can have as much more information as it pleases to call for from moment to moment, by an operation equivalent to 'changing a gramophone record'.

Computing machines of the electronic digital variety are highly novel. Historically, the first such machine produced its first useful result a bare eight years ago. However, a flourishing industry is already based on their production. Their full potentialities have still to be explored. One most interesting field of use covers the control of machine tools. This is liable to have a very far-reaching effect on a small-scale production, that is, where but one piece of some very complicated shape, such as a three-dimensional cam, is required. Reductions in working time of the order of 10 to 20 to one have been claimed for devices of this kind. It might be noted that the most advanced pioneering design of this kind was inspired by an *acute and irremediable shortage of skilled labour* in the district where it was produced.

Such is the fourth topic which can properly be included in 'automation'.

Automation and Technological Change

In view of the likely spread of automation, it is desirable to have a clear picture of how the technological world reacts to changes of a novel character.

A novelty can frequently be postulated years in advance of its first *economic* reduction to practice. But a latent period of very variable length has to be presumed before this occurs. The first economic reduction to practice may be caused by one of three changes in the interim period.

First, new methods or materials may have become available, This was so in the case of Whittle's gas turbine: creep-resisting stainless steel turbine blades were available to Whittle, whereas they had not been to his precursors in the field.

Second, the economic factors may have changed in the interim, as witness the relative success of automation in Detroit 20 years after its relative failure in England. The history of retractable undercarriages for aeroplanes is another instance. Such undercarriages reduce drag. They were first proposed when air speed and drag were both too low for the reduction of the latter to be worth while. As air speeds rose, retractable undercarriages became economic.

Thirdly, a special purpose may emerge which requires the use of the novelty irrespective of cost. Such was the history of silicone greases which found their first big field of application for certain special purposes in nuclear energy plants.

Once the first economic reduction to practice has taken place a number of consequences follow. Duplicate devices cost much less than prototypes. Costs come down, and what was barely profitable to start with becomes progressively more so as time goes on.

Secondly, designers crowd into the new field opened up. Early designs are minutely examined and prove on analysis to be relatively clumsy and expensive. A wave of redesigning takes place, and relevant gadgetry multiplies without limit. Costs accordingly come down still further.

Thirdly, as costs are progressively reduced, so new fields of application become more profitable. A novel technical process is thus converted into a technical snowball. Nothing succeeds like success.

Something of this sort is bound to occur with automation, whichever of its sub-divisions we choose to consider. We are scarcely at the beginning of 'redesign for automation' as distinct from "production by automation"; nor has factory redesign for automation yet had much consideration. In fact, 'reunification in continuity' cannot occur if components of manufacturing processes requiring 'reunification' are conducted at opposite ends of the plant site.

In its physiology a factory resembles an arthropod, such as a lobster, rather than a verbetrate, such as a horse. Its rigid framework or skeleton—its building—is outside, not inside, its functional organs. The framework cannot grow accordingly. A verbetrate and its skeleton grow in proportion to one another. An arthropod must 'moult' at regular intervals if it is to grow. Factory growth is not dissimilar. At intervals a new factory is built on an adjacent site and the old factory dismantled. We must accordingly wait for a new generation of factories in order to see the full consequences of automation. The regeneration of factories is, however, a continuous process like the human birthrate. For this reason we cannot expect to see spectacular results. For a long time to come the evidences of automation must be restricted to a patch here and a path there, as at present. Any technology which operates with long-life facilities cannot by and large produce short-term revolutions.

The Social and Economic Consequences

One of the surprising disclosures resulting from the interest shown in the social and economic consequences of automation, and the controversies it has aroused, is that no one appears to have any clear ideas how social and technological changes accommodate themselves to one another. Does advancing technology create employment or cause unemployment ? Has society a normal state to which it tends to revert ? Is it one of massive structural unemployment from which it has to be rescued ? Or is it one of full employment and inflation which has to be kept in bounds ? Alternatively, has society no stable state ? Is employment oscillatory and highly disturbed ? Are technological changes the sort of disturbances which cause oscillation ? There seems no accepted doctrine on any of these points.

Let me first consider the integration of technical and social changes.

Advancing Productivity

Whatever the consequences of automation may be, they cannot differ from the consequences of any other advance in productivity — for an advance in productivity is what automation amounts to.

Consider, for example, that in 1956 we enjoy *per capita* all the goods we consumed in 1906 and many more besides— automobiles, television sets, antibiotics, and other products which scarcely existed or did not exist at all 50 years ago. How is it possible to make, per head of population, all that we made 50 years ago and more besides ? It *can* only be because we can make the type-products of 1906 with less effort than was entailed 50 years ago, the displaced productive-power being redeployed for the manufacture of, for example, television sets. In fact, productivity per head has been advancing at about 1½% compound interest *per annum* for the last 50 years, with a marked acceleration recently ; the present figure is thought to be as high as 3%. How is it, then, that we enjoy full employment in 1956 ? Is it a coincidence ? Were there no television, would all the people employed in manufacturing television sets be unemployed ? If people could not buy television sets, would they buy vacuum cleaners instead ? Would there be higher employment in other trades accordingly? Has technology a reservoir of ideas for consumers' durables, capable of absorbing redundancy, caused by advancing productivity, for an indefinite time to come ? If any substantial thinking along these lines preceded public interest in automation, few of its conclusions seem to have found their way into circulation in time to be of any use in discussions of the matter.

In these circumstances, I can only give my own conclusions in the form of assertions.

The first is that technology as such can create neither employment nor unemployment. The only component of society which can create employment or unemployment is the money market.

The second question, concerning the normal state of society, I must answer with a second and qualified assertion. Are we talking of the pre-Keynsian or post-Keynsian economies ? Keynes claimed to know the answer to the cause of trade cycles, and F. D. Roosevelt gave his exponents a chance to prove it. In the opinion of most people, they succeeded in doing so. We are all Keynsians now. In the pre-Keynsian era the normal state of society *was* one of deflation and structural unemployment. The investment rate tended, as it were, to bump along the bottom of its permissible value. An economy in this condition *is* near the instability level and oversensitive to disturbances which cause oscillations of the 'boom and slump' variety, with their attendant unemployment in the slump phase of the trade cycle. In the post-Keynsian era the investment rate has, by central government and banking action, been kept up to a permanent level high enough to maintain the economy in a reasonably safe, stable, non-oscillatory condition. At the price of some inflationary writing off of old capital, capitalist society has achieved the stability denied to it by Marxist theory. It appears that full employment, with some inflation, has come to stay ; and most people are prepared to tolerate the inflation for the sake of the increased stability. There has been a real change. To the cynic murmuring 'Plus ça change, plus c'est la même chose', I would reply that change *can* be real. The change from the mercantilist to the laissez-faire outlook was a real change. That from the pre- to the post-Keynsian outlook is another.

The third question concerning the supply in perpetuity of novelties such as television can be answered in the affirmative, even though these novelties will not necessarily be technical in character. A sewing machine is a novelty for the housewife who has not got one. So long as man's standard of living is distributed over a broad range and there is an adequate investment and employment policy, the raising of the standard of living of the lower levels in society to that of the higher can absorb any skill displaced by advancing productivity. The only issue therefore turns upon how society accommodates itself to advancing productivity locally and over short periods.

Labour Turnover—Retirement and Recruitment

Retirement and recruitment would appear to be the normal mechanism whereby technology is integrated with social change. As happens too often where the under-endowed social sciences are concerned, the need to provide an authoritative answer to snap-questions in this field finds us short of the requisite information. The following picture cannot however be very far from the truth.

First, a working career begins some time after school-leaving age at 15 - 16, and continues, in the majority of cases, until retirement at some time before 65. As a first approximation, it thus occupies 50 years.

There is accordingly a basic labour turnover of 2% due to human senescence. It is much greater locally, however, since not many men remain in the same job or employment that they entered as apprentices or on leaving school. If the average man

were to hold five jobs in the course of his working career, representing an average of 10 years apiece, the basic turnover in any factory would be 10%.

Women frequently retire from work when they marry and start a family, and resume it again on completion of their principal life occupation as mothers: two 10-year periods, covering the ages of 15 - 25 and 45 - 55, are likely to be spent in employment. This would correspond to turnovers of 10% if only one job were followed in each period. If two jobs were followed, the turnover would be 20%.

It is not always easy to check these figures, owing to the inadequacy of available statistics. It is known, however, that there are two classes of employed labour, one known as stable labour, usually of a skilled variety, and the other as unstable labour, the former having a relatively low rate of turnover, the latter a relatively high rate. Gross turnover averages the two.

For example in the U.K. the figures for all industry for July, 19596, were: [1]

	Male	*Female*	*Total*
Engagements and additions (per cent)	2.7	4.2	3.2
Discharges and losses (per cent) ...	2.8	4.4	3.3

These are monthly figures (five weeks in this case), and it will be noted that they balance to within 0.1%. The annual turnover appears accordingly to be of the order of 30%. It seems clear that a spontaneous turnover of something like 30% can easily accommodate a 3% displacement caused by advancing productivity at constant production. (In fact production is not constant but increases, so that any redundancy due to advancing productivity is less than the 3% advance in productivity itself.)

The Changing Pattern of Society.

As technology advances, the requisite pattern of skills changes across the years. Old skills become obsolete, new skills are called into being. Jobs that once required skill are performed by the unskilled, while jobs once the function of the unskilled are transformed so that skill is required in their performance.

In one sense, however, the net changes are in the direction of increasing skill of a kind at the expense of un-skill. The proportion of society's members occupied as hewers of wood and drawers of water is dwindling, a higher standard of education is required of all, and the proportion of the highly skilled tends to increase progressively.

This process is frequently described as the upgrading of labour. If a hard task once performed by human muscles is transformed so that it is effected by a machine requiring a skilled operative, we say that the labourer has been upgraded into a machine minder.

It is important to remember that the word 'upgraded' has a statistical and not an individual meaning. The individual labourer may only rarely be upgraded; he more usually changes his job. Some labourer's son, however, ceases to follow his father's calling, and becomes a skilled operative.

Let it be supposed, then, that some skilled man is made redundant by a new technical process. What exactly happens to him as an individual? In the first instance, he merely changes his job and fills one of the vacancies that are constantly occurring owing to retirement of fellow workers possessing his skill, or to their leaving their job spontaneously for some reason or another. If his skill is 'on the way out', the consequence of a long-term trend, then there will be no demand for apprentices to replace those who retire and he will find himself a member of an aging group of craftsmen, the demand for whose skill diminishes as their numbers dwindle.

This is the broad picture. What we require to know is how a process, which is clearly self-balancing in a statistical sense, operates equitably in its effects upon the individual. It is assumed above that the craftsmen displaced by a new technical vacancy can be redeployed. Can they, however, be redeployed without personal hardship?

Strain and Stress

In engineering jargon a strain is a displacement, a movement of material; stress is the force that always appears as a concomitant of strain, resisting it, restoring the *status quo*. For a given strain, stress will be higher in a material such as steel than in one such as aluminium or lead.

There are some advantages in carrying this terminology over into sociological nomenclature. Social strains will then denote displacement, dismissal, redeployment, and change of status or income, while the concomitant stresses will be psychic forces and their manifestations, resentments, strikes and so on.

In the process of integrating social change with technological change, we must assume that there will be strains. The problem is simply how to keep down the stresses.

Consider, for example, the strain represented by displacement and redeployment of labour. What will determine the magnitude of the stress?

Stress will clearly be least if the individual can be redeployed at the same occupation and wage within the same factory. Even in this case, stress will not vanish, for there will be anxiety until the issue is clear, and anxiety, even if finally relieved, will leave a scar of resentment.

Stress will be greater if the individual has to seek alternative employment. There will be anxiety until it is secured, and there may be dissatisfaction with its nature. This dissatisfaction will be least if alternative employment is secured in a locality no farther from home. It will be much greater if change of employment involves change of district. Stress will be greatest if a change of occupation is involved, especially if a skilled craftsman, after a period of unemployment, secures employment as a labourer at a lower rate of pay.

Much of the report of the Department of Scientific and Industrial Research on automation is concerned with techniques for keeping the stresses down, with particular reference to re-training of displaced personnel. How realistic re-training will prove to be

1. *Ministry of Labour Gazette*, July, 1956, page 271.

on the material plane, it is difficult to foresee. It is hard to teach an old dog new tricks. Its advocacy, however, is real on the psychological plane.

It is not enough that justice be done: it must also be *seen* to be done. It seems but fair that if a man's occupation is transformed by a machine, then he should be given a chance to be the man to work the machine if he can; that if his skill is in danger of redundancy by other men's actions (the whole complex of actions by management, designers, technologists, scientists and others responsible), then these actions should include re-establishing him at his old earning power; and that the cost of doing so should be part of the *true cost* of the technical development rendering him redundant. It is perhaps worth noting that this approach is advocated by scientists and engineers, and is a welcome sign of their sensitivity to the problems they help to create.

It might be helpful to introduce here two concepts currently used by the communication engineer: that of 'signal' and that of 'noise'. A signal is whatever can be recognised and acted upon. Noise is the component of a signal which contains no information and cannot therefore be a basis for action. Social hardships may be considered in these terms. A certain amount of hardship is inevitable as the price of being alive. It is always happening to someone. Should society treat it as signal or as noise?

Thus in the United Kingdom, for example, 20,000 people die by violence every year: 5,000 on the roads; 5,000 from industrial accidents; 10,000 from domestic accidents — they fall out of windows, out of bed, downstairs, into the fire or off ladders; they electrocute themselves in the bath, are struck by lightning or eat poisonous mushrooms; they commit suicide. These accidents occur after the traffic police, the factory inspector and society at large have done whatever is possible to ensure that they do not occur. They are what cannot apparently be helped. Yet each of them means hardship or sorrow for someone. I suggest that they should be regarded as society's 'noise level' of personal misfortune. In this sense, the misfortune of the man in a thousand who, under present conditions of full employment in the United Kingdom, may be unemployed for so long as 10 weeks if he loses his job, is 'noise'. When society has paid his unemployment relief, it has done all that can be done. Whether the victim reacts with fortitude or self-pity, there seems little his neighbours can do about it, save by way of sympathy and small kindnesses.

But structural unemployment, by contrast, is not 'noise'; it is a 'signal' calling for action.

Back to Automation

With a firmly established picture of how the integration of social change with technological change takes place, we can return to our original subject. Automation does not differ generally from any other advance in productivity, and everything that has been said here applies not only to automation but to any other technological advance. The stresses associated with them will be determined by the manner in which they are dealt with, and this will not be affected by the technological origin of the strains.

All that remains for consideration therefore is the number of occasions on which strains will arise. Reverting to the engineer's concepts: will the social strains arising from automation be signal or noise? There already seems to be sufficient evidence to assert that they will be indistinguishable from other background strains.

Let us consider the components of automation in order.

Transfer machining is now some 10 years old and its introduction seems to have been as strain-free as that of any other new process. It has so far been employed in the context of consumers' durables, such as automobiles, and its use has been initiated to clear production and bottlenecks in factories where total employment is rising. It is difficult to see why it should cause more strain in the future than in the past.

Automatic assembly has been employed for so long in the electric-lamp bulb industry that its introduction elsewhere should not cause alarm. In the context of the electronics industry (printed circuit assembly techniques) any displacement it effects is likely to fall on young female labour having a very high rate of spontaneous turnover. There should, therefore, be no serious problem here.

Control engineering techniques are already widely employed in the chemical industry, which uses them pre-emptively. This industry characteristically employs but few direct operatives as compared to maintenance engineers. It is only the plant operators who will be affected by automation, and they are more likely to be assisted than displaced. (Note that an autopilot does not replace the pilot of an aeroplane. It merely lightens his task.) Where but few are employed, few can be displaced.

Data processing and computerised accountancy is the most interesting of the four cases calling for analysis, because a digital computer is a more fundamentally novel and powerful device than any other employed in automation. Accountants are likely to be the victims, if there are any, particularly accountants concerned specifically with the mechanics of book-keeping. It must be remembered in the first place that such accountants are not generally employed in local concentrations; with rare exceptions (such as, in the U.K., the City of London, and the National Health Insurance offices in Newcastle) they never constitute local industries to be compared with coal-mining, ship-building, or automobile manufacture. They are distributed over all industry. Secondly, the computerising of an office routine is a long and tricky business. Those qualified to operate it are at present an almost microscopical handful, and suitable recruits are very difficult to find. Computerisation, therefore, can scarcely proceed very fast, even if the production difficulties involved in manufacturing computers on a large scale could be overcome. Thirdly, accountants and clerical workers

generally are at present in short supply ; there is little prospect of automation doing in this field more than it has tended to do in every other field where it has been introduced, that is, to clear a bottleneck.

It must be remembered that a new facility presents the user with three opportunities : to perform a traditional task more cheaply, to perform it better for the same cost, and to undertake some non-traditional task. The importance of computers, in the opinion of those who have used them most, appears to lie in the second and third of these opportunities.

Having considered the four types of interaction which automation may have with industrial society, we may conclude this section with a review of those industries which are unlikely to be affected by labour displacement as a result of automation. In each case the fourth type of automation, computerised accountancy, will be disregarded, since clerical work is more or less the same everywhere and has been dealt with above. With this qualification, industries may be split into the following groups :

1. Industries where automation is unlikely to be applied, for example, agriculture, mining and transport. All are highly mechanised, but the sub-species of mechanisation which pertains to them does not seem to be automative in character or to lend itself to further automation.

2. Highly mechanised industries where automation seems unlikely to be applied, other than as an occasional auxiliary. The textile industry based on natural fibres is one such case. No doubt textile machinery can be improved and productivity raised, but that does not by itself constitute automation. Transfer does not seem to be a problem involving many operations, though the auto-doffer for ring spinning frames has something of the transfer idea in it. Control engineering techniques seem unlikely to be more than useful auxiliaries. They are unlikely to cause labour displacement.

3. Industries basically similar to the chemical industry, that is, those which have low direct labour costs and where automatic control techniques are more likely to improve the performance of the operatives than to render them redundant. All continuous processes, and plants operating them, such as sheet rolling mills, would fall into this class, together with brick-making, iron and steel works, coke ovens, gas works, electrical generating stations, and so on.

4. Industries such as soap and beer manufacture, where an initial operation resembling those of the chemical industry (group 3), for example, soap boiling or brewing, is followed by a highly mechanised stage (similar to one of those in group 2) for distribution purposes, for example, soap tableting and packaging, and beer bottling.

5. Trades involved in the manufacture of heavy capital goods, such as, housing and structural engineering generally, and shipbuilding.

The trades most likely to be affected are those already aware of their involvement with automation, that is to say, those engaged in manufacturing consumers' durables. These trades are already highly sensitive to problems of redundancy, as they are always the ultimate victims of a slump or a fiscal policy based on restricting consumption. In this connection, it should be noticed that the recent increase in purchase tax on automobiles and the restriction of credit for hire purchase in the U.K. has dislocated the employment situation in the British motor industry far more than the installation of transfer machining ever did.

Benefits and Long-Term Effects

Having looked on the dark side of automation, in so far as it has one, and decided that it is not so dark, let us dwell briefly on its advantages. First, it must be regarded as an advance in productivity, and welcomed accordingly as the type-contribution to a continually rising standard of living. In this sense, it is but the current phase of a process at least two centuries old. Its adoption will be enforced by competition. Whether misinformed theorists like it or not, automation will be adopted. The issue, as the DSIR report clearly demonstrates, is whether we put it to good use or not.

It may, however, be more far-reaching than a mere current advance in productivity. In a section of his book specially written for the American translation, Friedmann [1] considers automation, without however defining it very clearly (he seems to think mainly in terms of transfer machining). He concludes that it may offer an avenue of escape from the problem whose answer partly baffles him — the monotony of sub-divided repetitive tasks, and the elimination of joy in work from the worker's daily routine. Personally I doubt this being so, and yet there is a sense in which I believe Friedmann may be right if one takes a long enough view of the matter, though the solution which he foresees will not, I think, any longer be called 'automation' when it becomes manifest.

I believe, in fact, that our society is emerging from the worst of the machine age into better times, and that automation is a symptom of this partial emergence. There has been a trend to 're-unification in continuity' for some time, and automation is an aspect of it. Increasingly, continuous processes replace batch processes, and observational skills are becoming as highly sought for as manipulative skills. The passing of the old-fashioned foreman, old-fashioned manager, and old-fashioned boss are palliatives. The tyranny of the assembly line (which actually affects only a minority, though a vocal one, of workers) is becoming less unendurable, while in patches it is being mechanised and integrated — though only in patches and that but experimentally. If Friedmann is correct in his hope and I am correct in my belief, then our society has escaped spiritual destruction, though perhaps by a very small margin.

1. Georges Friedmann, *Industrial Society*. The Free Press, Glencoe, Illinois, 1955.

The Labourer

My own concern has from the start been with the man who is not going to be affected by automation rather than with the man who is — the hewers of wood and drawers of water who do the heavy labouring work for a society which does not know how to lighten their task. Typical of such are the coal-miner, the stevedore, the road-mender, and the builder's labourer. The miner has the dirtiest, hardest, and most dangerous occupation in the world. The stevedore ranks second. The road-mender and the builder's labourer are subject to every hazard of wind and weather. No matter how highly mechanised the task may be, the unpleasant and dangerous character of coal-mining will remain so long as coal is black and must be won underground, subject to the hazard of a roof that may fall or of fire-damp that may explode. What is going to be the effect on the community of harbouring within it a dwindling minority of essential workers who live a life unknown to the rest of the community, subject to dangers unshared by the rest of the community ?

The life of a fisherman is hard; it is also dangerous. But the fishing fleet does not sail in and out at the beck and call of a siren, and if the sea is dangerous it can also be beautiful. There is a ' lure of the sea '. Is there a ' lure of the mines ' ?

Two problems require to be faced, and we know the answer to neither. The first is how to lighten the essential burden of the miner's occupation. The second is how to integrate his outlook with that of the rest of the community. These are indeed baffling problems.

Science and Industrial Society

The function of science is to observe, explain and expound. It is not its function to take sides in controversy, for controversy only arises where authoritative exposition is lacking because science has nothing to say. In so far as scientists have any part to play in controversy, it can only be by placing the facts at the disposal of all contestants so that they may see the nature of their dispute more objectively. In writing of automation, I have endeavoured to keep this ideal before me. The main problem our society has to resolve is the inability of man at any level of specialisation to undo his mythical fall, and re-integrate himself. This fact is basic. There is no difference in this respect between worker and manager, and the fall is replicated at every level, manifests itself in every occupation, and repeats itself every day as, grasping at a more abundant life for ourselves, we initiate developments whose consequences we cannot foresee. It appears, moreover, that this cannot be otherwise : as we re-integrate ourselves in one respect, we are disintegrated in some other. The Marxist attempt to re-integrate society economically merely disintegrated whatever tender growth of liberalism once existed in Russia. The political liberalism of Western Europe merely connived at gross exploitation of man by man in the economic sphere.

Our current dangers lie in the division of society into two hostile sub-groups, a managerial group relatively free and a workers' group relatively unfree. Their unfreedom and their discontents are however of a sophisticated character. Although the cleavage follows the old line of demarcation between rich and poor, riches and poverty are not the current issue between the contestants, for the standard of living in the West is now so high that poverty in the old sense of the word scarcely exists. The current issue, barely acknowledged to be so, is more freedom in relation to work. It may well be the case that as freedom spreads, any residual bondage is felt to be increasingly savage. It may be the case that where bondage is least, there it is felt to be most insufferable.

For this reason I regard our current discontents as sophisticated. They could not occur at a lower level of evolution. On all such matters, however, we need far more information than we yet possess. We cannot give manager and worker an objective picture of one another, because we do not know enough about their interactions. We can, however, recognise components of hostility, of suspicion, of anxiety, and its attendant aggression. These are psychological realities. Their causes may not correspond to material realities. Therefore, it seems, good will and trust must to some extent be dependent upon objectivity springing from dispassionate accumulation of facts.

Automation is a material reality. It is also a psychological reality. The two realities do not necessarily correspond. As a material reality automation is a source of wealth. As a psychological reality it is a source of fear. May this present essay help to show how the two realities can coalesce, for such is the function of the social sciences in their application. They have few functions of comparable importance to the contemporary world.

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THE IMPACT OF THE PRODUCTION ENGINEER UPON THE STRUCTURE OF WORK AND PAYMENT IN INDUSTRY

by Dr. ELLIOTT JAQUES

Presented to the London Section of the Institution, 18th October, 1956.

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During the War he served in the Royal Canadian Army Medical Corps as a psychiatrist, concerned with officer selection and repatriation work. At the end of the War he decided to remain in England, and joined the staff of the Tavistock Institute of Human Relations. In 1952, *he left the Institute and went into private practice as a psycho-analyst and industrial social consultant. Since that time, much of his consulting work in industry has been done with the Glacier Metal Company Limited. Some aspects of this consulting work are surveyed in this Paper.*

I

THE introduction of changes in production methods as a result of production engineering development work may often modify the available jobs and the payment attaching to those jobs. Sometimes the modifications are of the order of a radical change in organisation, and in structure of payment.

Production engineers are familiar with these changes. It is my purpose in this Paper to explore in detail not only how these changes occur, but how the size of the changes can be measured and their effects anticipated at the planning and design stage.

II

The Fundamental Character of Production Engineering Work

I would like to suggest that the basic feature of production engineering work is that of discovering how to modify what is discretionary in an operation, so that the discretion is removed, and the operation becomes increasingly prescribed. Let me illustrate what I mean.

If you tell me to tighten a series of nuts, you may leave it to me to judge when they are tight enough. You leave it to my discretion. If, however, you provide me with a torque wrench, you remove the discretion I had previously to use, in judging when I had tightened the nut just enough. Without the torque wrench, I have to work by touch and feel; with the wrench, I tighten as quickly as I can, knowing that I cannot over-tighten, or if I do, that it is the fault of the wrench, and not my fault.

I have picked a simple example, because all the important elements can be seen in it: in particular the elimination of some elements of the touch, or feel, or judgment, which in the absence of the development would require to be exercised by the operator — but by means of the development, get built into the tooling for the job.

You will be familiar with this kind of development work. Elimination of discretion in this way frees the operator to work more quickly and easily at the job. But, of course, there is some loss of flexibility in planning, and repetition of the same kind of work is necessary in order for the requisite tooling to be economic.

Having taken one simple example, let me now extend the list in order to illustrate my meaning in more detail. The kind of discretion used in machine operation includes: hand-grinding of tools to get a good cutting angle; hand-feeding of tools to take the right sized cuts; setting a number of tools on a lathe so as to carry out a number of operations at the same time; using marking-off equipment; interpreting

(not just reading) complex or incomplete drawings; using tackle in slinging heavy work ; using leaf gauges to judge looseness or tightness of fit ; judging adequacy of finish ; deciding whether on balance work at the borderline of the prescribed tolerance should be sent forward or rejected ; judging temperature of melts by colour ; keeping a steady and adequate rate of flow in pouring a melt — the list is legion.

By way of contrast, you will recognise how you introduce limitations on discretion by your design of tooling, and of standardised forms. Drill jigs eliminate the discretionary component involved in marking-off for drilling ; standardised fixtures eliminate discretionary improvisation in holding work during operations ; automatic temperature controls cut out the need for judging temperature by colour ; standardised drawing forms eliminate the need for repeated design of tools or parts with similar basic components ; automatic feeds and stops eliminate need for judgment in how fast and how far to take a particular operation ; go-no-go gauges and clock gauges remove the judgment in the feel required for the use of micrometers at fine limits.

The process of prescribing discretion reaches its highest point in automatic machine tools. Here most, if not all, the operating discretion is built into the machine. But a new type of discretion is thrown up — that of increasing complexity of judgment required in setting, and in machine minding, that is to say, in keeping the machines running as efficiently and with as few and as short stoppages as possible.

III

I will assume that this interplay of judgment or discretion, and of the non-discretionary or prescribed elements of production work, is an old and familiar story to you. If this assumption is warranted, then I would like to show you how this way of looking at your work can be turned to account in assessing the social effects of production engineering work. To do so, I would like first to present to you a new method of work study for assessing the level of work, or of responsibility in jobs, by paying close attention to the prescribed and discretionary components.

This method of work study was developed in the course of recent work — carried out over the past eight years in a consulting capacity with the Glacier Metal Company—in which a yardstick for measuring level of responsibility in jobs, without reference to skill, training, experience, etc., has been developed. The type of consulting relationship is a special one, and is of relevance to an appreciation of the results I shall present: I term it " social-analysis ". The particular conditions of the relationship with the firm are as follows :-

(a) my part-time contract is determined only by unanimous agreement of the Company's Works Council — on which are represented shop stewards, all grades of staff, and top management ;

(b) I do no work on my own initiative, but work only at the request of individuals or groups at any level in the Company ;

(c) my relationship with the members with whom I work is a completely confidential one ;

(d) I present to the members concerned my analysis of their own views, not discussing anyone else in the organisation with them, nor making recommendations ;

(e) nothing is reported of these discussions, except at the request of the individuals or groups concerned, and in conjunction with them. The material I shall present to you has been reported by this means to the Company's Works Council, and hence has been cleared for publication outside the Company.

The significance of the particular relationship is that I am able to have discussions with individuals or groups in the organisation in which a sorting out of their own views can take place without any executive action necessarily arising as a result of the discussion.

The material I shall report arose from a request from one of the representative staff committees to consider with them their views about staff status and salary arrangements. They were clear that they wanted status and grading and pay to be connected with the level of responsibility or work carried by individuals, the problem being to get a yardstick to make such assessment possible. The work initiated by them was then picked up at their recommendation by the Company management, and by the other representative bodies in the firm, including the shop stewards' committee and the elected representatives of the three grades of staff.

One of the results of this work has been the discovery that what they experienced as level of responsibility or level of work in their jobs could be measured by what we termed the "*time-span of discretion*". The time-span of discretion may be defined as the maximum period of time during which any employee is authorised and expected to exercise *discretion* in his work without that discretion being subject to review by his superior. It differs from existing methods of work analysis in that it does not depend on any opinion of those assessing the work as to the rating to be assigned to factors like skill, training, experience, length of service, accuracy required, complexity of work, etc. It is based on an objective analysis of any job of work as established by the manager in charge of the job, on behalf of the employing organisation. That is to say, we analyse real jobs as they are organised in specific firms at any given time. Stereotypes about a job being big or small (because jobs of that title — say a typist, or draughtsman, or foreman, or machine operator, or managing director, or toolmaker — have come, through custom and tradition, to be regarded as being of a certain size) have to be resolutely set aside, and replaced by *direct analysis* of how the work in a job is actually organised and carried out at the present time.

IV

The essence of this time-span method of measurement is that we discriminate between the prescribed aspects of work and the discretionary aspects, to which I have already referred. As you will have noted, the prescribed content of a job consists of those elements of the work about which the member is left no authorised choice. From the production engineering point of view, the prescribed content refers to those aspects of the job which are built into the tooling and are not left to the judgment of the operator. The discretionary content of work, on the other hand, consists of all those elements, examples of which I have mentioned, in which the choice is left to me, the operator, as to how I go about my job. You will recall my examples of interpreting drawings, using leaf gauges, judging adequacy of finish, and so on.

In industry, we seem to attach the feeling of the responsibility we carry to the discretionary content of our work, and not to the prescribed content. That is to say, we feel weight of responsibility in terms of the discretion or judgment that we are called upon to use.

The actual measure of responsibility can be got at in terms of the maximum period during which I am relied upon to be using my own judgment. This period can be assessed by discovering the mechanisms which my manager uses to review the quality of the discretion or judgment which I use. Was my judgment good or bad, adequate or inadequate, just good enough, or just sub-standard? Having discovered how and when these review mechanisms work, we can ascertain the maximum period of time I could continue to exercise my own discretion without review. It is the discovery of these review mechanisms which has proved most difficult in practice. It would appear that in industry we know very little about the actual techniques we employ to control the quality of judgment of subordinates.

The methods a manager employs to review his subordinate's use of discretion can be divided into two main categories: *direct review* by himself (or by someone acting on his behalf); and *indirect review* through the medium of reports on the effectiveness of work done reaching the manager from sources outside his direct control.

In order to discover these review mechanisms, it has been found most satisfactory to look at the matter in a negative way. This negative — or indeed, upside-down — approach consists of considering how long it would be possible for me to exercise *substandard discretion* continuously and in all aspects of my work, before my manager could become aware of what was going on. That is to say, a search was made for the methods by which a manager would discover not the serious errors of judgment — these were usually easy to detect — but errors of judgment of such a degree as just to matter economically, but individually not to attract attention unless looked for. The reality of the time-span instrument is that it measures the longest periods of time that it is, in fact, executively possible for a subordinate to be piling up just unsatisfactory work, without the Company knowing about it.

I might add that I have now completed over 900 analyses of individual jobs of all kinds at all levels, ranging from unskilled hourly-rated work, to the work of senior executives, with time-spans ranging from periods of a quarter of an hour to an hour, up to periods of five to seven years.

V

The major significance of this time-span of discretion was its apparent connection with what members of the Glacier Metal Company experienced as fair rates of pay. Over a period of some eighteen months' analysis of jobs, I began to notice that members whose jobs worked out at the same maximum time-span, tended in discussion with me to refer to the same amounts of money as fair payment for their work. If they were paid above these amounts, they felt they were getting more than their share; if they were paid below, they felt they were being badly done by; if they were being paid at this amount, they felt they were being fairly treated within the general levels of pay obtaining in industry.

This finding, which was empirically discovered and not based on any prior theory, came as a distinct surprise. It appeared as though members of the Company, without knowing it, were intuitively operating a common scale of payment, whether wages or salaries, in terms of their intuitive judgment of level of work carried.

I have since been able to test these findings in other organisations. I have not a sufficient number of results as yet to make a conclusive statement; but I can say that a high degree of consistency obtains —that is to say, members in other companies state the same levels of pay as being fair for similar time-span levels. These results are suggestive of the existence of an unrecognised national wage and salary structure, payment according to which would be experienced as differentially fair and proper. My own view would be that it is deviations from this intuitively recognised structure that lead to disputes over differentials in industry.

This structure is illustrated in Diagram 1.

Moreover, a natural status and grading structure related to level of work has now been observed, as well as a wage and salary scale. This structure is illustrated in Diagram 2.

VI

The significance of this time-span instrument from the point of view of the production engineer, is that it is possible to examine the development work in train from the point of view of how it will change the discretionary content of the jobs under consideration. It is possible to analyse these changes at the planning stage, because the essence of your development work is that you do know precisely

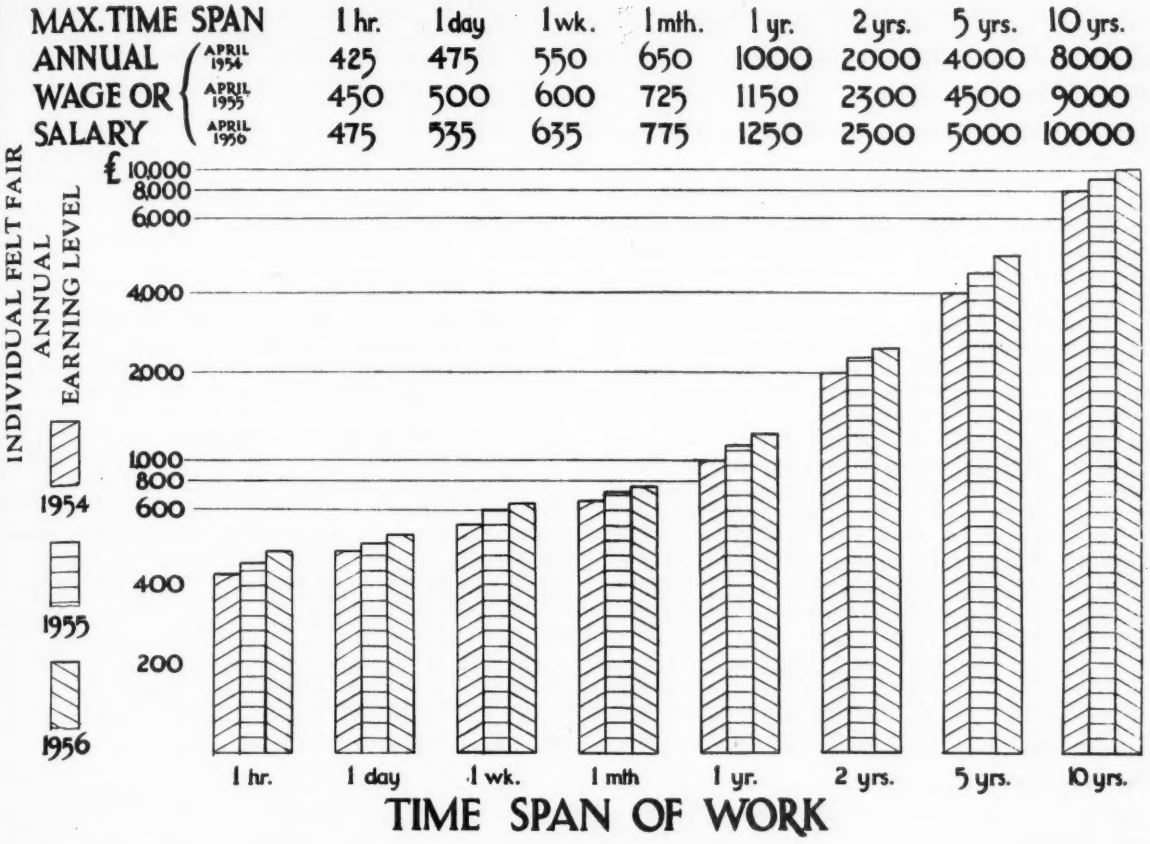

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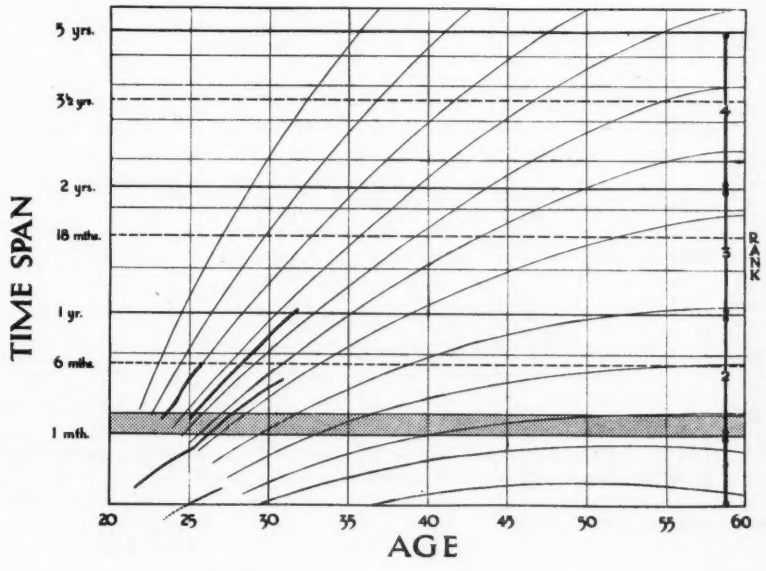
Diagram 1



(right)
Diagram 2

what operations formerly requiring judgment are now to be prescribed by the development of tooling, jigs, fixtures, or indeed of machine tools, which you are considering. It is possible to do a very detailed analysis of discretionary and prescribed content of the new job of work at the design stage on the drawing board.

Once you have sorted out the discretionary and prescribed aspects of the newly-developing job, it is then possible to discern what kinds of review mechanism will be required in order to assess and control the quality of discretion being used by the operator. From this information about discretionary content and review mechanisms, the range of level of work in the newly emerging job can be assessed.

On the basis of such an assessment, the impact of the production engineer on the structure of work *can* be evaluated. It is possible to assess with a fair degree of accuracy whether the developments are going to throw up a higher level of work, or leave the level of work unchanged, or reduce the level of work in a department. Given this assessment, it becomes possible to anticipate, from changes in level of work, the changes in the rates of pay which are likely to be stimulated by the changes in methods, and any changes which will be required in the level of capacity of the operators employed.

The effects upon ancillary jobs need to be assessed in the same way—drawing office work, maintenance, supervision and management, and, indeed, the effects upon the related engineering jobs — time study, methods engineering, estimating, and planning.

This latter point, that of the relationship between level of work thrown up by particular methods and the level of skill of the operators required to do this work, is an important complicating factor which I would now like to touch upon.

VII

I had noted that the time-span of discretion carried by individuals showed a regular type of progression if one examined the work they did at various points in their careers. This progression suggested that there might equally be discovered regular patterns in the progress of earnings of individuals. I therefore plotted the course of the earnings over the past ten years of between two and three hundred persons. These were corrected to a common level for changes in the wages index, so that only increases due to merit would be shown, and cost of living and increases due to movements in the wage index would be cancelled out. The results of this procedure are illustrated in Diagram 3. You will

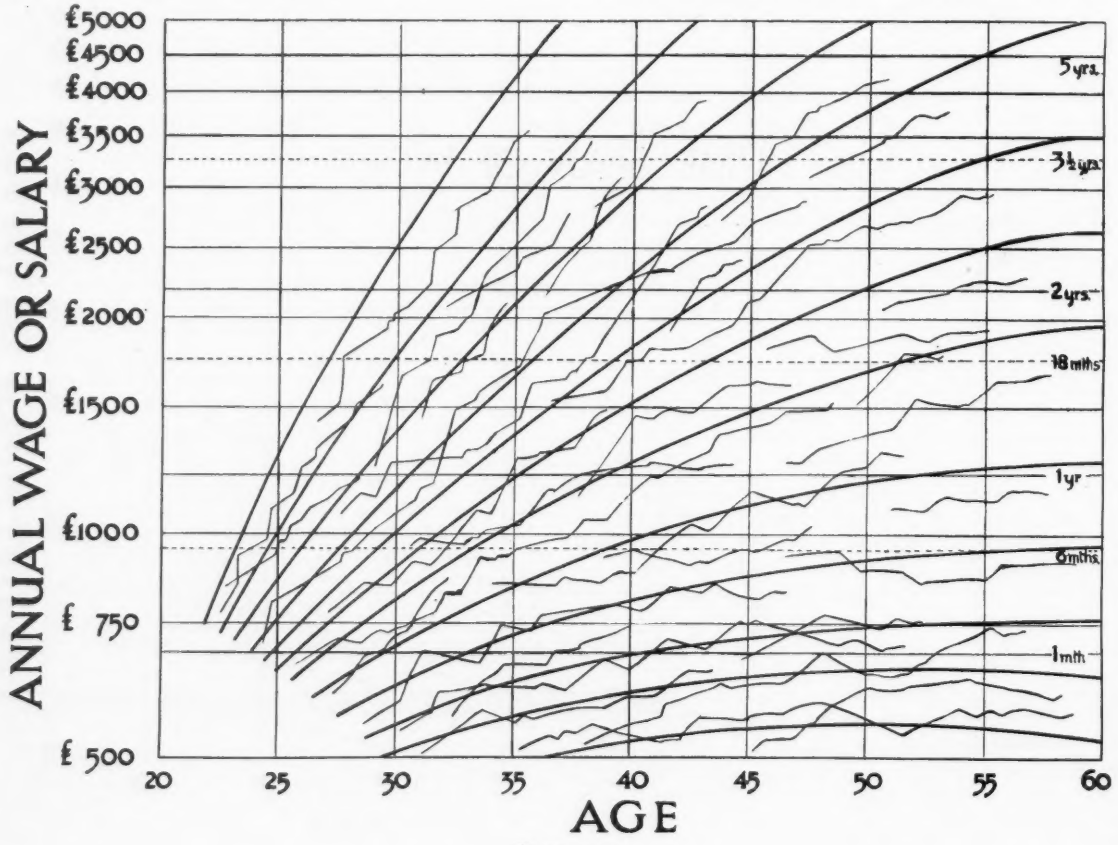


Diagram 3

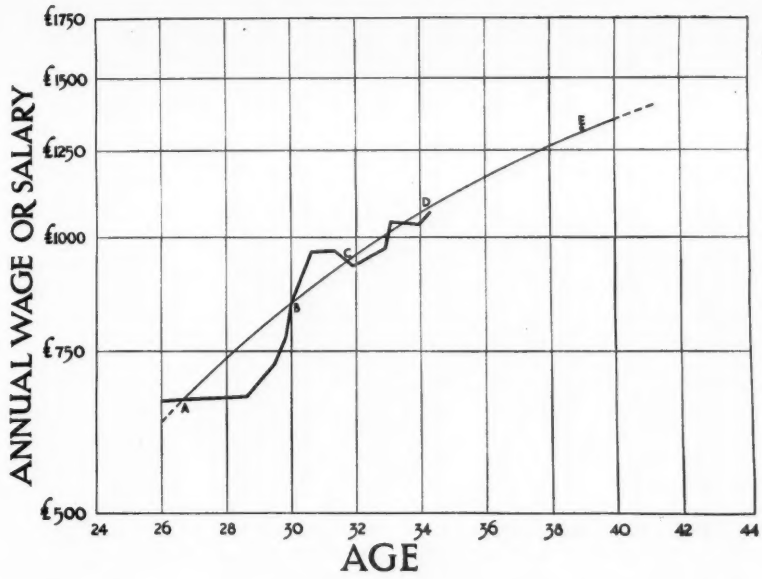


Diagram 4

note that there is a regularity in the progression of these earning curves, as though a field of force is operating. The direction of lines in this field of force I have tried to indicate by the smooth black curves. Subsequent analysis of these curves has given the following results.

The black normal earning growth curves have turned out to be well-nigh perfect fitting Gompertz curves, that is, biological growth curves. This fact suggests that we are here dealing with a *normal biological growth phenomenon,* the growth of capacity to work.

The most important finding has been that individuals whose earnings are consistent with their level of work, and follow the normal pattern, tend to describe themselves as satisfied with their earning progress. Deviations of between 2% to 10% from this normal pattern are noticed. Deviations between 10% and 15% are strongly objected to, and individuals begin to complain and even to seek other jobs. Deviations between 15% and 20% are consistently accompanied by strong feelings of resentment, and eventually — unless the situation is corrected — by seeking a change of job.

The above results may be illustrated in Diagram 4 showing the progress of one individual. Between C and D he stated that his earnings were fair, and analysis of the time-span level of work showed that his earnings were consistent with the level of work that he was carrying. From A, however, he began to feel increasingly dissatisfied with his earnings, and was preparing to leave the organisation, had a discussion with his manager, and decided to stay after being given a considerable increase. By B, he felt that he was progressing satisfactorily again, and between B and C he considered that he had received differentially more than his fair share. He predicted his salary in five years' time as E.

With regard to this latter point, it is a striking finding that individuals tend to predict their own future earnings in terms of this normal growth pattern. Whether a person describes himself as ambitious and out for a great deal of money, or rather more interested in his work than he is in the amount of money he earns, each one nevertheless seems to state an earning level that lies on his earning growth curve.

The degree of consistency of individual reactions to deviance from their normal earning progression may be demonstrated by the fact that I now find I am able to construct the *actual earning curves* of individuals from their current time-spans and their description of their feelings about their work. The fact that it is possible to do reconstructions of this kind illustrates what I feel to be the reality of these normal biological growth processes in terms of progression of capacity to carry responsibility. Further evidence of this consistency can be shown if we consider the age-payment curves of four large organisations.

Where consistent progress is lacking, it becomes possible to predict and anticipate morale problems in groups. Thus, in a group of subordinates where the maximum level of work was at the one-year time-span level in Diagram 2, as soon as one of the members reached this level, the whole of the group began to feel dissatisfied. The morale problems arose regardless of the fact that there had been no explicit salary levels stated, nor had the top levels of work been measured and recognised.

The point I would emphasise is that these results suggest that individuals exert considerable pressure to obtain work consistent with their current level of capacity, and to obtain payment consistent with that level of work. When they are under-employed — they will feel underpaid relative to the work they feel

capable of doing. And because money is specific — they tend to voice under-usage complaints in terms of too low pay.

VIII

If we now consider both the time-span measure of level of work and the growth curves of individuals, we can tease out in more detail what happens when production engineers introduce changes in methods.

Let us take first the case where the level of work is increased by the changes in methods; that is to say, new discretionary elements such as are thrown up by the machine minding aspects of automatic machines are introduced, and the periods of review of discretion are longer than they were under the previous methods. You can note from Diagram 2 how such a change gives greater opportunity for those of higher capacity, and whose capacity is growing. We can see also that the young employees, even of lower capacity, will not be too badly off. But the middle-aged and older employees on these flattened curves will find that their capacity cannot increase to the level of the new work, and that therefore they are likely to prove untrainable for it. The requirements of the situation, therefore, are that new and adequate rates of pay are introduced in line with the new levels of work, and that the employment of the older and lower capacity group be considered with them.

Taking the opposite case — that usually referred to as "de-skilling", where much of the discretionary content of the work becomes prescribed by new tools, jigs or fixtures, or automatic control mechanisms — the following situation may also be noted by reference to Diagram 2. The level of work will be cut away from the men of higher capacity, and they will be disgruntled; the middle-aged and older men of lower capacity will find the new situation tolerable; the younger men of lower capacity will also find the situation unsatisfactory because, whereas previously they could intuitively feel opportunity for progress, they will now intuitively realise just as clearly that the previous opportunities have been removed. In order to work through changes of this kind, what is usually done is that the more highly skilled men whom the organisation wants to keep are moved to other jobs and are replaced by employees of lesser skill. With the instruments described, it is possible to anticipate the extent of the difficulties likely to be met.

X

If the above instruments for the assessment of level of work and the progressing of individuals prove reliable and valid, then the following procedures would be possible :-

(a) the calculation of existing levels of work in departments where methods were to undergo change;

(b) assessment of the range of level of work available under the new methods;

(c) assessment of the spread of capacity among the individual members of the shop or department.

From these assessments, the degree of dislocation in terms of both individuals and payment structure can be anticipated, and concrete and specific plans for coping with these changes can be drawn up and discussed in detail with those concerned in advance of the changes taking place.

I would add one final note on a different but related topic. It may be of interest to you to know that there is some experience to suggest that analysis of new methods at the design or development stage, in terms of sorting out the discretionary from the prescribed content of the work, can be of value in the design and development work itself. If this analysis is carried out in sufficient detail, it becomes possible to ask in a systematic way, whether it is in fact desirable that this or that item which remains discretionary should be left to the discretion or judgment of the operator. Such consideration may often suggest further development work which might improve design from the engineering point of view. Contrariwise, it is possible to examine what is being prescribed in the method, to decide whether the rigidity so obtained is in fact preferable to the flexibility that might be achieved if that particular part of the operation were left to the discretion of the operator.

Furthermore, systematic consideration can be given to the methods to be used for controlling the prescribed aspects, and for assessing the quality of discretion. Systematically looked at in this way, thinking about design may be facilitated with respect to the possible building into the design of review and inspecting mechanisms.

RESEARCH PUBLICATIONS

A number of copies of the following Research publications are still available to members, at the prices stated :

Machine Tool Research and Management	10/6
Report on Surface Finish, by Dr. G. Schlesinger	15/6
Practical Drilling Tests	21/–

These publications may be obtained from the Production Engineering Research Association, "Staveley Lodge", Melton Mowbray, Leics.

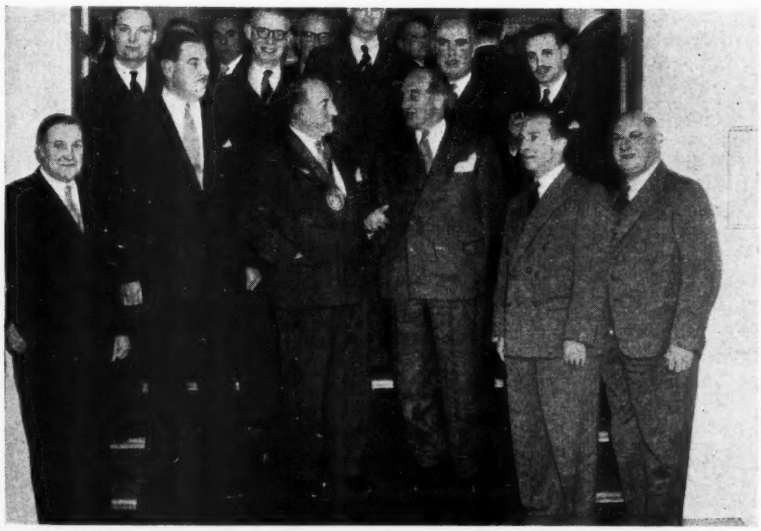
JOURNAL BINDERS

Strongly-made binders for the Institution Journal, each holding 12 issues, may be obtained from Head Office, 10 Chesterfield Street, London, W.1, price 10/6 each, including postage.

Mr. E. J. Pode Honoured

Mr. E. Julian Pode, Member, Managing Director of The Steel Company of Wales, was installed as Honorary Freeman of the Borough of Port Talbot on 7th March last, and was presented by Alderman Graham Griffiths, Mayor of Port Talbot, with a silver casket and an inscribed scroll to mark his admittance as a Freeman.

Mr. Pode first went to Wales in 1926, when he joined Guest Keen & Nettlefolds as an accountant at their Dowlais Works. He is a Past President of the South Wales and Monmouthshire Section of the Institution.



Reading Section Dinner

The Reading Section Annual Dinner, which took place on 1st March last, was attended by over 50 members and guests. It was a most enjoyable occasion presided over by the Chairman, Mr. H. A. Richardson, and the Section were privileged to have as guest of honour the President of the Institution, Mr. E. W. Hancock, M.B.E.

A large measure of the enjoyment was due to the musical entertainment arranged by Mr. Richardson and Mr. Mott.

New Appointments

Mr. B. E. Cook, Member, has been appointed a Special Director of Fisher and Ludlow Limited, in control of production at Fisher and Ludlow Limited, and also Nuffield Metal Products Limited.

Mr. T. H. Hall, Member, has relinquished his position as Works Manager with The Head Wrightson Machine Co. Limited, Middlesbrough, and has taken up an appointment as General Works Manager of Head Wrightson (Teesdale) Limited, Thornaby-on-Tees.

Mr. H. W. Harper, M.B.E., Member, who was General Manager and a Director of Fisher and Ludlow Limited and was responsible for production at the Company's factories throughout the Midlands,

has resigned. Mr. Harper joined the Company, which is now part of the British Motor Corporation, in November, 1946, as General Manager of the Erdington factory, and became special director in 1949. He was formerly an executive of A. V. Roe Limited. He is a member of the District Advisory Committee of the Midland Regional Board for Industry.

Lt.-Col. R. L. Kapoor, Member, has been appointed Production Engineer with Hindustan Aircraft (Private) Limited, Bangalore.

Mr. A. Miller, Member, has been selected as a Machine Tool Expert on the Development Council for Machine Tool Industry of India.

Mr. C. L. Old, Member, Principal of the Wolverhampton and Staffordshire College of Technology, will take up a post in September as Group Adviser, Education and Training to Vickers Limited. Mr. Old is a Past Chairman of the Institution's Education Committee, on which he still serves.

Mr. F. Whitehead, Member, has relinquished his appointment as General Manager with the Chatwood Milner (Speke) Organisation, and has been appointed General Manager, Metal Equipment Divisions, at Darlaston and Wrexham with Rubery Owen & Co.

Mr. J. F. A. Bryen, Associate Member, has been seconded from the Sales Division (Production Control Department) of the British Tabulating Machine Company Limited, to the Works Division, to devise and effect the application of computer techniques to production control systems in the Company's production factories.

Mr. R. S. Bumstead, Associate Member, has recently relinquished his appointment as Assistant Director of Studies, R.E.M.E. Officers School, Arborfield, to take up the position of Production Control Officer at 202 Base Workshops, Royal Canadian Electrical and Mechanical Engineers, Montreal.

Mr. S. A. Clodd, Associate Member, Works Manager, Radio and Television, has been appointed an Executive Director of E. K. Cole Limited. He joined the Ekco war-time factory at Aylesbury, Bucks., in 1941 as Material Controller, and transferred to the Company's main organisation at Southend in 1945 as Production Controller. He was appointed Works Manager in 1950.

Mr. B. W. Charman, Associate Member, has been appointed General Manager of Rhoden Partners Limited. Since Mr. Charman joined the Company in 1955, he has been its Chief Draughtsman. Before this, he held senior appointments with engineering companies specialising in the design and manufacture of automatic transfer machines, and materials handling and processing equipment.

Mr. G. Crook, Associate Member, has been appointed Principal of the Tamworth College of Further Education, Tamworth, Staffs.

Mr. T. S. Dickinson, Associate Member, has been appointed Chairman and Managing Director of The General Engineering Co. (Radcliffe) Limited. Mr. Dickinson joined the Company in 1921 was appointed Technical Director in 1944, and joined the Board in 1946.

Mr. T. H. Jeens, Associate Member, has recently taken up an appointment as Personal Assistant to the Works Director of British Typewriters Limited, of West Bromwich.

Mr. R. Gunnell, Associate Member, has relinquished his position with the Skefco Ball Bearing Co. Ltd., and has taken up an appointment with Shell-Mex & B.P. Limited as a Lubricants Supervisor.

Mr. R. W. Marks, Associate Member, has relinquished his position as Sales Manager of Muschamp Engineers Limited, and has taken up an appointment as General Manager of Manwood Miller & Co. Limited and Associated Companies. For many years he was the Chief Production Engineer of the Traction Control Departments of Metropolitan-Vickers Electric Co. Limited and was later Works Superintendent of Lockers Engineers Limited and Air Maze Limited.

Mr. M. H. Moore, Associate Member, has taken up an appointment with the International Business Machines Corporation, New York, at the Poughkeepsie Plant as a Quality Engineer.

Mr. S. Jenkinson, Associate Member, has now left Pultra Limited and has taken up an appointment as Unit Manager with the Glacier Metal Co. Ltd., Kilmarnock.

Mr. G. A. Winterburn, Associate Member, has recently taken up a position as General Manager of Platt do Brasil Maquinas Texteis Limitada, Estado do Sao Paulo, Brasil, for a period of at least three years.

Mr. Bernard Armstrong, Associate, of the Management Studies Department of the Portsmouth College of Technology, has been selected to join a British delegation which is visiting the United States for three months under the auspices of O.E.E.C. The scheme is a European Productivity Agency Project on "Training in the United States for European Teachers of Business Administration".

Mr. R. P. Bull, Graduate, has recently been promoted to the position of Chief Education and Training Officer at A. A. Jones & Shipway Limited, Leicester.

Mr. K. G. Catlin, Graduate, has relinquished his appointment with Hoover Limited, and has now taken up an appointment as Technical Assistant with the Atomic Weapons Research Establishment, Aldermaston.

Mr. V. Gokhale, Graduate, has taken up a position as Planning Engineer with the De Havilland Engine Co. Limited, Leavesden. Until recently Mr. Gokhale worked in the production jig and tool drawing office.

Mr. H. M. Roberts, Graduate, has recently taken up an appointment with Production Improvements (Birmingham) Limited, as Northern Area Manager.

Hazleton Memorial Library

ADDITIONS

Klaf, A. A. **"Calculus : Refresher Course for Technical Men."** *New York, Dover Publications,* 1956 (*i.e.* 1944). 431 *pages.* $1.95.

Klaf, A. A. **"Trigonometry : Refresher Course for Technical Men."** *New York, Dover Publications,* 1956 (*i.e.* 1944). 629 *pages. Diagrams.* $1.95

These two textbooks are suitable for persons with an elementary school knowledge of mathematics who want to "teach themselves"; and for technical men who wish to revive their knowledge of the elements of the subjects. The subject matter is presented in question and answer form. There are problems with their answers.

Kretzman, R. **"Industrial Electronics Handbook."** 2nd edition. Translated from the German by Harley Carter, and revised by H. E. Kater and D. J. Mitchell. *London, Philips' Technical Library,* 1956. 298 *pages. Illustrated. Diagrams.* 42s.

The book deals in particular with electronic circuits employing highly evacuated or gas-filled discharge tubes, e.g., the amplifying tube used in radio and television engineering, photocells, cathode ray tubes, X-ray tubes, and gas-filled rectifying tubes with or without grid control. The first part describes the various types of tubes and their basic circuits, and the second a series of electronic devices for industrial purposes, e.g., electronic timers, electronic speed and temperature control devices, resistance welding control devices, electronic motor control, and some electronic apparatus for special purposes. The author is employed by N. V. Philips, of Eindhoven, and an appendix gives data of some of the principal types of Philips tubes.

Kozmetsky, George and Kircher, Paul. **"Electronic Computers and Management Control."** *New York, etc., McGraw-Hill,* 1956. 296 *pages. Diagrams.* $5.00 (37s. 6d.)

The book explains how electronic computers work and shows how they can be used to solve business problems. The administrative problems involved in the introduction of electronic methods in business are discussed, and advice is given on the selection of data processing systems for particular purposes.

Light, H. R. **"The Nature of Management."** 2nd edition. *London, Pitman,* 1957. 154 *pages. Charts.* 12s. 6d.

This edition covers the "Nature of Management" Paper of the Common Intermediate Examination in Management.

Naslin, P. **"Lecons d'Anglais Scientifique et Technique."** *Paris, Dunod,* 1956. 391 *pages. Diagrams.*

This book is intended for French students of technical English and, therefore, the explanations are in French, and there is no indication of the pronunciation of French words. But, because the book is for the most part a phrase book, any engineer who has "kept up" his school French, and now wants to improve his technical vocabulary should find it useful. There are lessons devoted to machine tools, to metal cutting and to workshop practice.

Palestrant, S. S. **"Practical Pictorial Guide to Mechanisms and Machines."** *New York, University Books ; London, Constable,* 1956. 255 *pages.* 4,000 *diagrams.*

All kinds of mechanical motions and holding devices are illustrated diagrammatically in this book which is similar to Herkimer's "Engineer's Illustrated Thesaurus". Brief explanations accompany the diagrams. The book might be a useful supplement to standard textbooks which are often inadequately illustrated.

Streets, Harold. **"Sutcliffe's Manual of Belt Conveying."** *Edinburgh, W. and R. Chambers Ltd.,* 1956. 150 *pages. Illustrated. Diagrams.*

The author is Technical Director of Richard Sutcliffe Ltd., manufacturers of conveyors and other mechanical handling equipment. A short historical account of the belt conveyor is followed by a description of the construction of belts, including those made of rubber and of P.V.C. ; by a guide to the selection of belts for different purposes, and by instructions on how to calculate the power necessary to drive a unit. There are full descriptions of gears and idlers ; of feed and delivery chutes ; and of methods of discharge. One chapter is devoted to various special purpose attachments (e.g., belt weighing machines and electronic metal detectors) ; and another to maintenance and repair.

Sykes, A. **"Gear Hobbing and Shaving."** *Huddersfield, David Brown Industries Ltd.,* 1956. 94 *pages. Illustrated. Diagrams.* 25s.

The author is Technical Director of the David Brown General Gear Division. "The purpose of this book is to describe gear hobbing and shaving machines as made by David Brown Industries Ltd. (Machine Tool Division), Manchester, and to give information which it is hoped will be useful in the operation of such machines."

Tegart, W. J. McG. **"The Electrolytic and Chemical Polishing of Metals in Research and Industry."** *London, Pergamon Press,* 1956. 129 *pages. Diagrams.* 30s.

This book is based on a report published by the Commonwealth Scientific and Industrial Research Organisation, Division of Tribophysics, of which the author is a member. Because the working of the process is not yet completely understood, no detailed treatment of the subject has been attempted, but a general explanation is given based on experimental observations. Besides this author discusses: the limitations of the techniques ; the apparatus required ; the necessary precautions ; the related process of chemical polishing ; electropolishing in molten electrolytes ; electrolytic and chemical polishing of some of the rarer metals ; the relationship between chemical and electrolytic polishing. Both laboratory and industrial techniques are described. There is an excellent bibliography which should be useful to both metallurgist and engineer.

Thompson, Laurence. **"The Challenge of Change."** *London, O.U.P.,* 1956. 127 *pages.* 5s.
The author, who attended the Duke of Edinburgh's Study Conference on "Human Problems of Industrial Communities within the Commonwealth and Empire", reports and comments on the discussions.

Udy, Marvin J. (editor). **Chromium—Vol. 1. "Chemistry of Chromium and its Compounds."** *New York, Reinhold; London, Chapman and Hall,* 1956. 433 *pages. Illustrated. Diagrams.* 88s. (American Chemical Society, Monograph Series.)
The book comprises 15 Papers by different authors. Accounts of the history, occurrence, chemistry and mineralogy of chromium are followed by an account of the physical and chemical properties of chromium compounds, two Papers on the industrial uses and the production of chromium chemicals; and accounts of the properties of chromium chemicals in relation to special industrial applications, e.g., in tanning, in the textile industry, in corrosion prevention, in the graphic arts, wood preservation, and in the oil and gas industries. There is a Paper on the manufacture and properties of chromium pigments.

Wilkes, M. V. **"Automatic Digital Computers."** *London, Methuen,* 1956. 305 *pages. Illustrated. Diagrams.*
"This book is intended to provide a general introduction to the principles underlying the design and use of digital computers. It covers the subject now generally known as logical design without entering into a detailed discussion of electronic circuit techniques. It also deals with the way in which programmes are constructed, and methods by which the machine itself can be made to assist the programmer in his task. Discussions of what operations need to be programmed to solve particular problems — the subject of numerical analysis — is outside the scope of this book. One or two machines are described in some detail . . ." The introductory chapter is an account of the development of the automatic digital computer and includes a description of Babbage's calculating engine.

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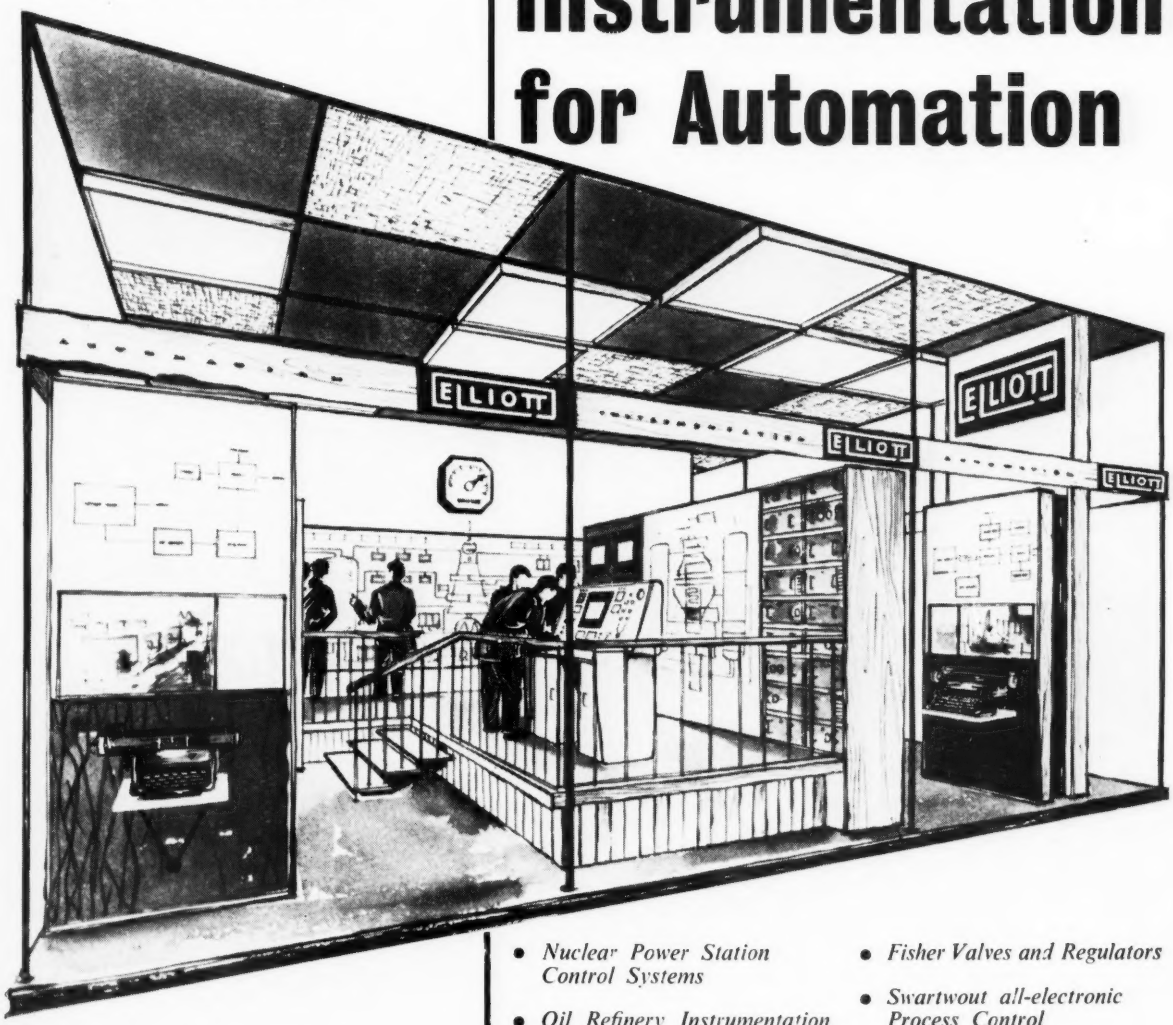

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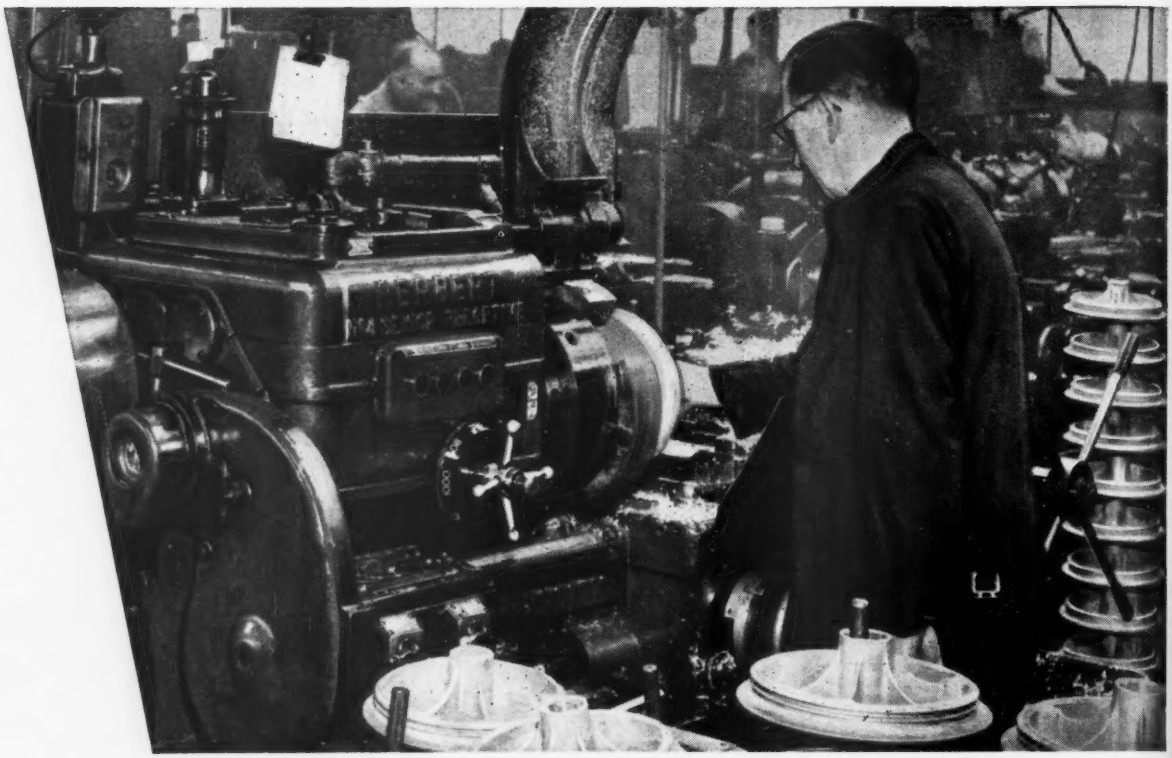

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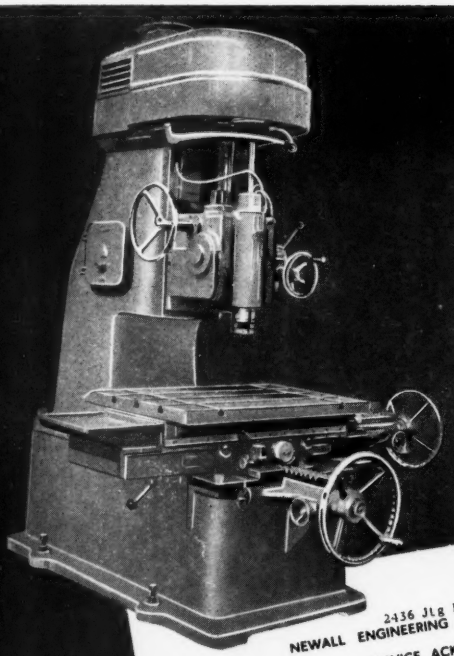

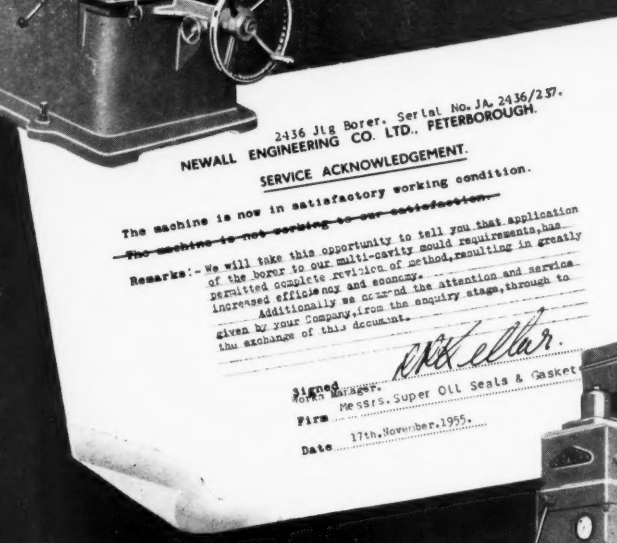
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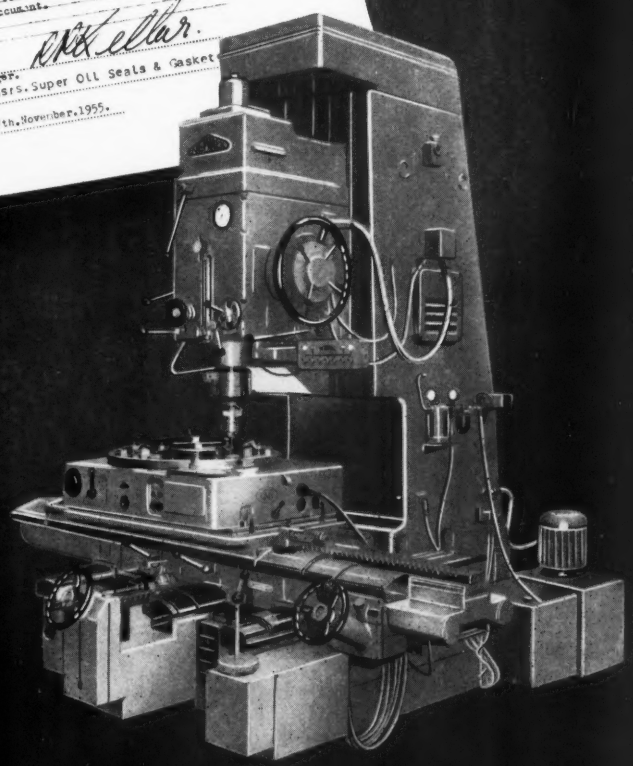

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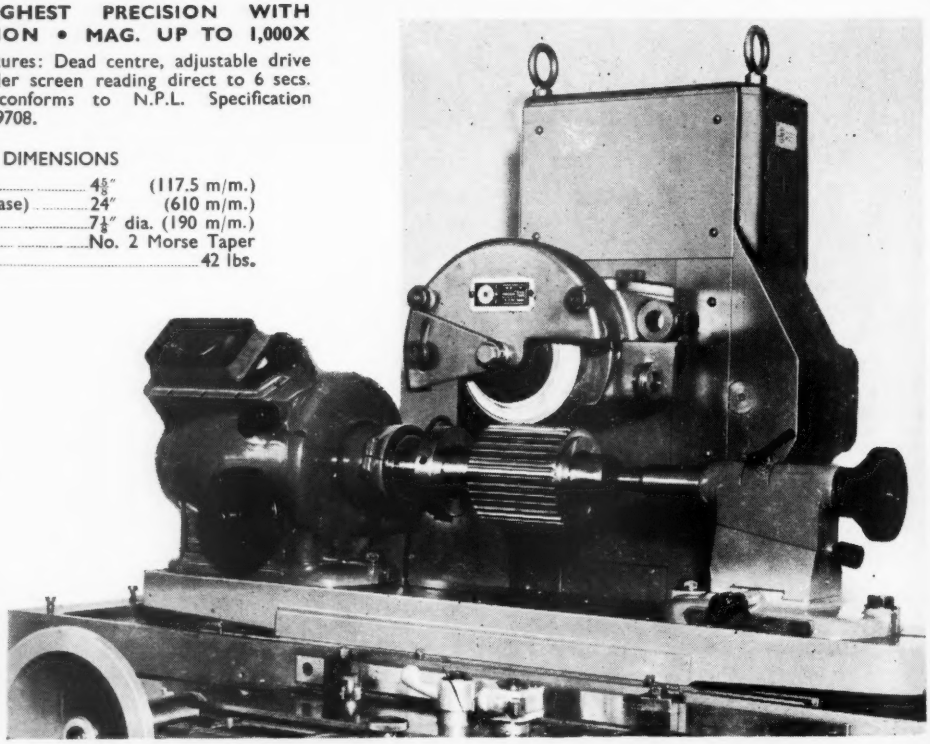

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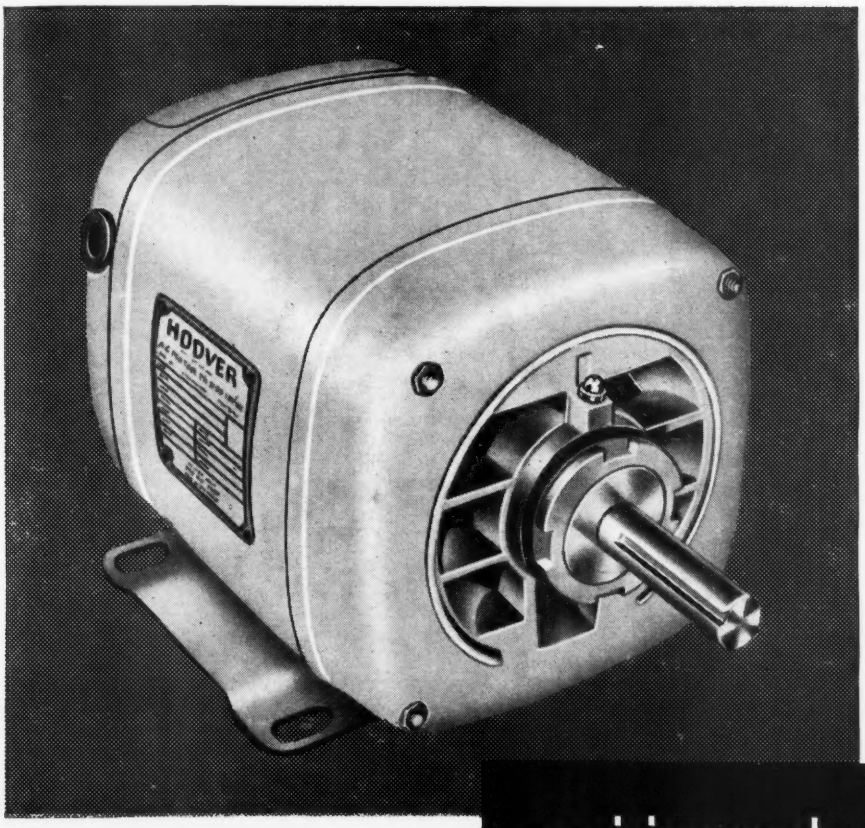

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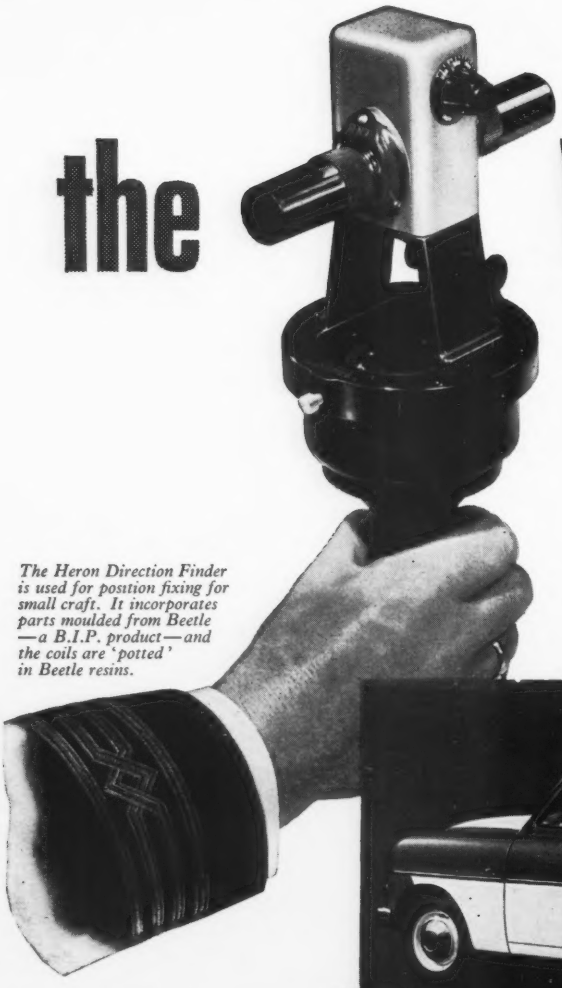

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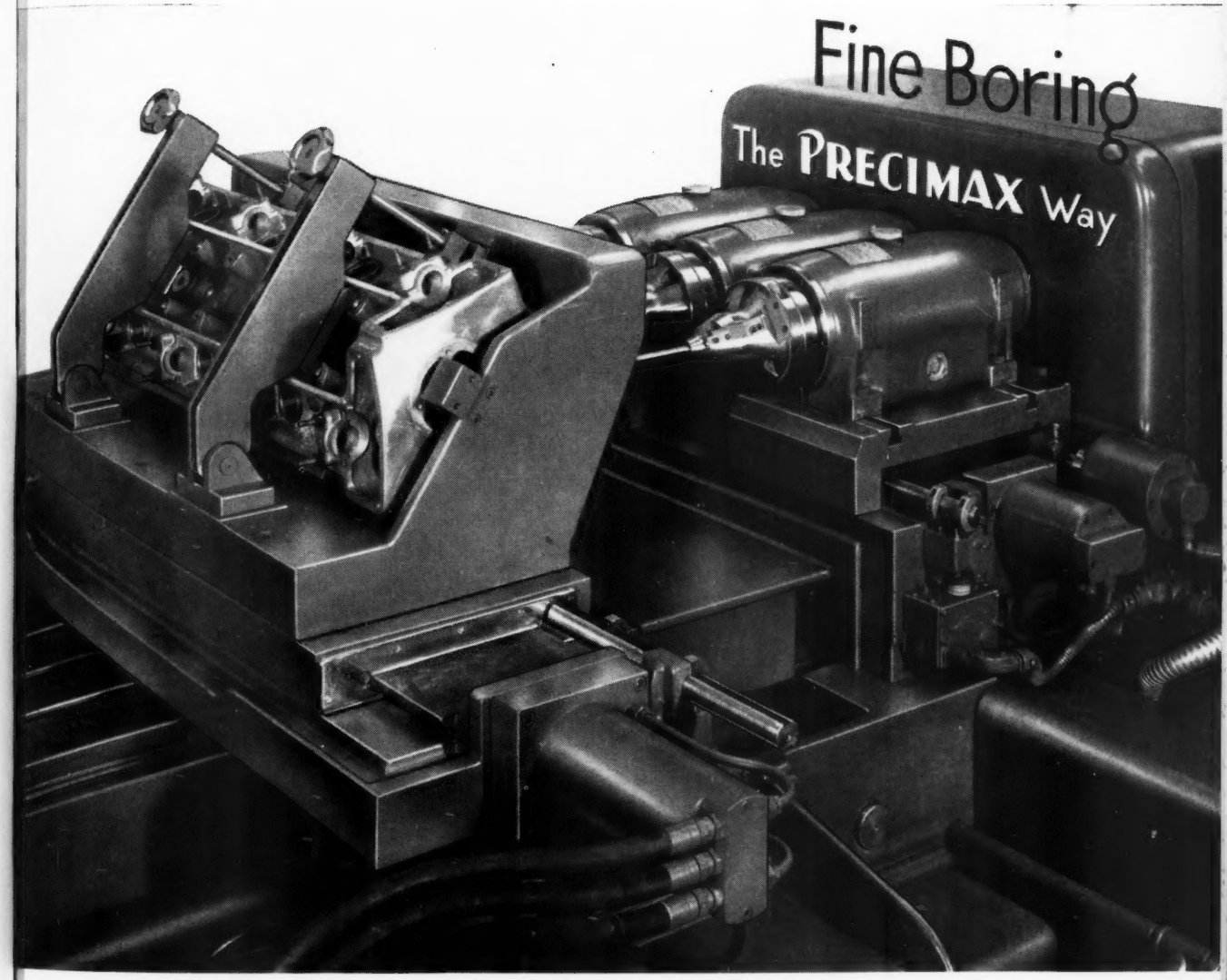

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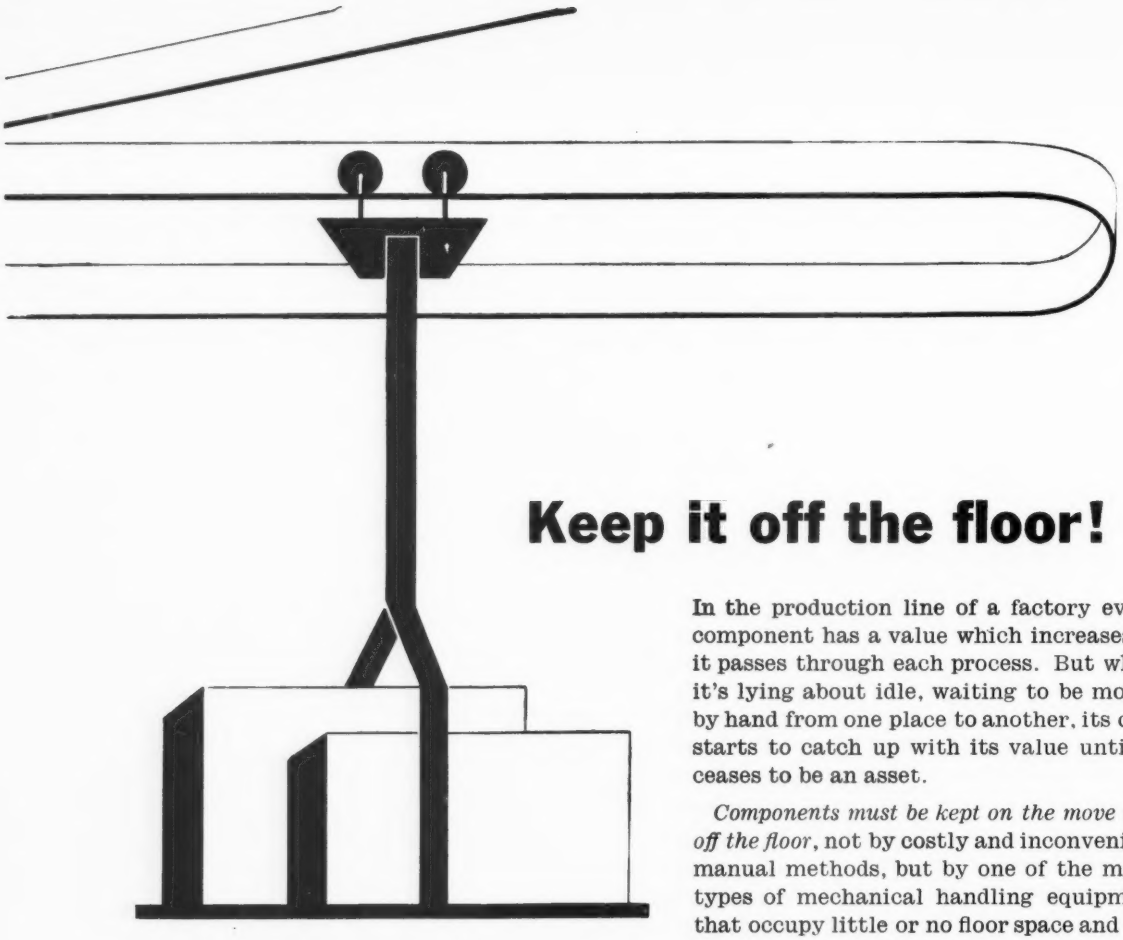

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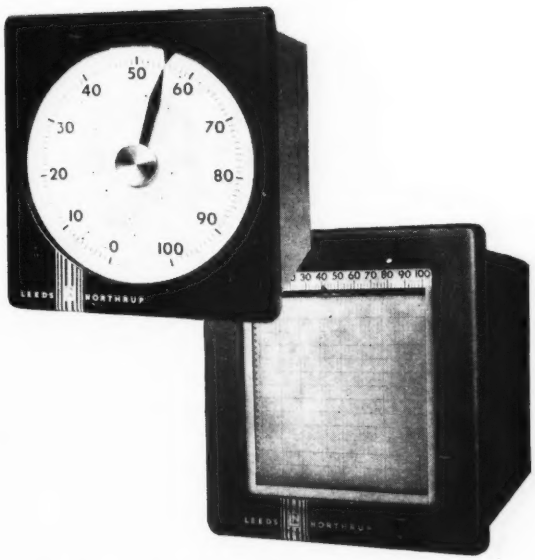

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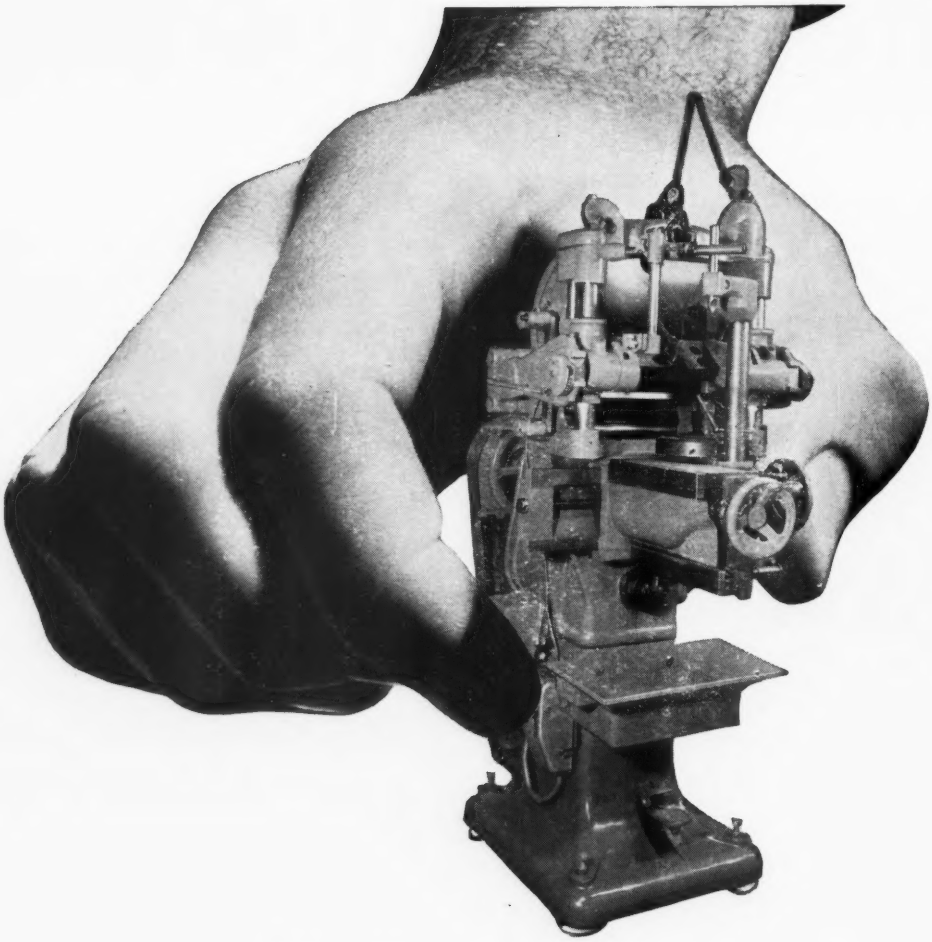

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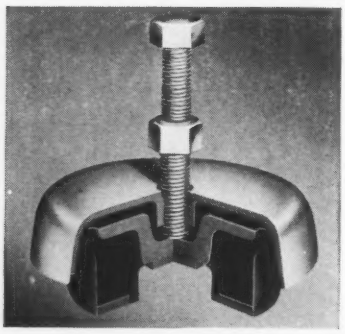

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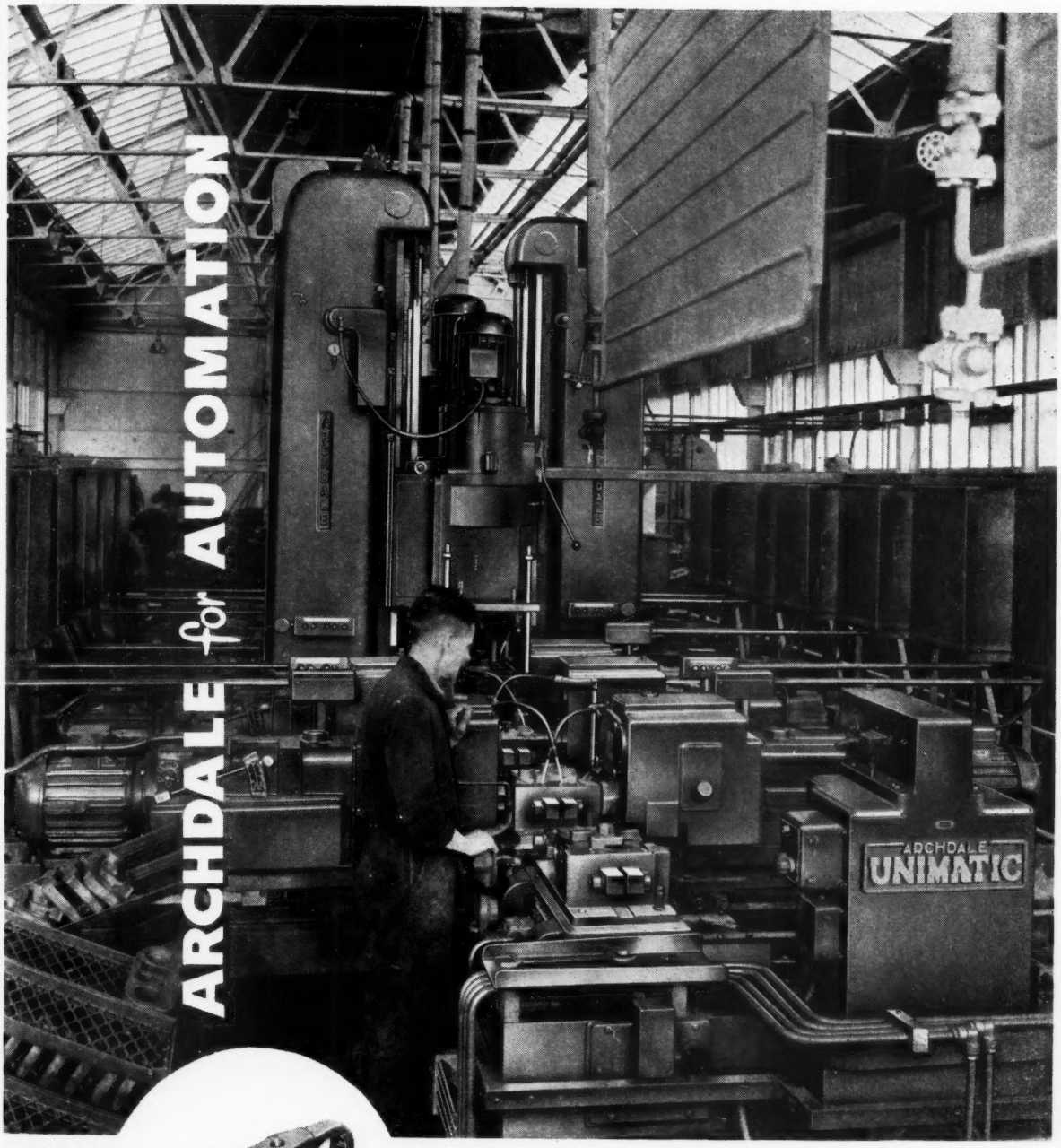

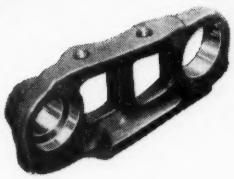

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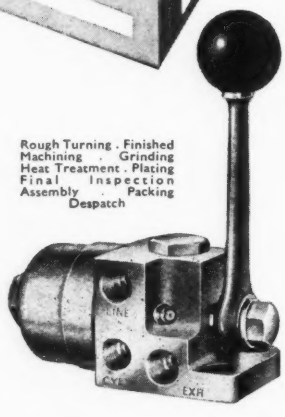

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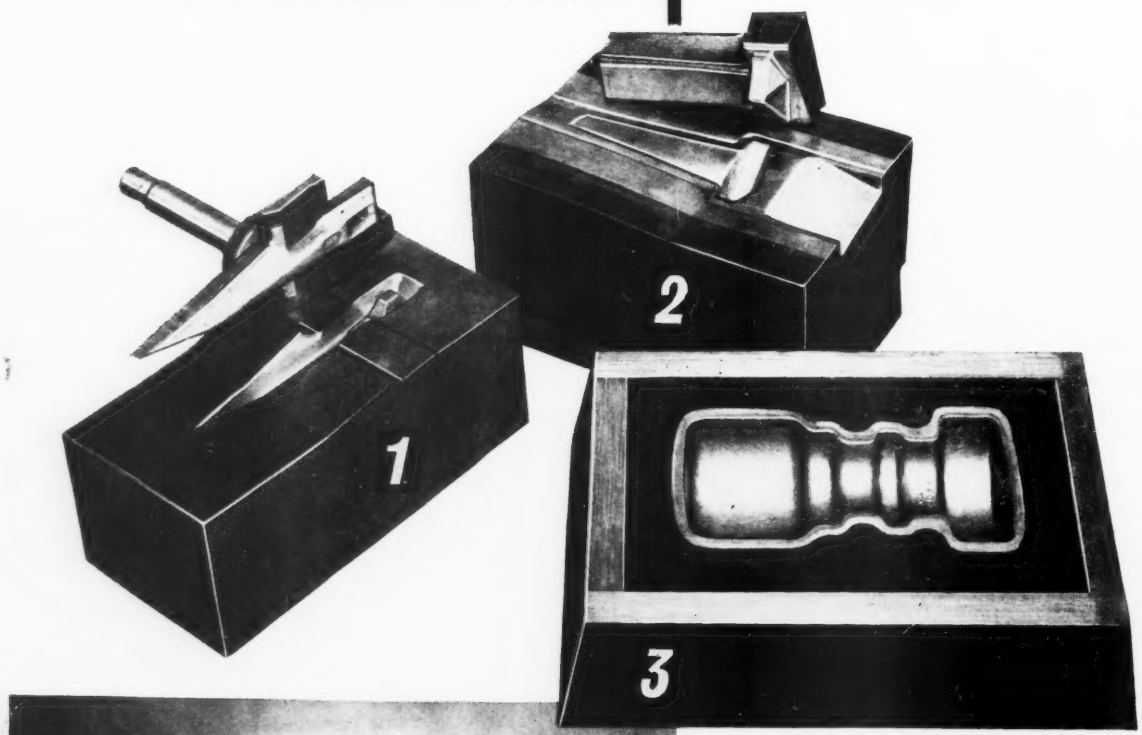

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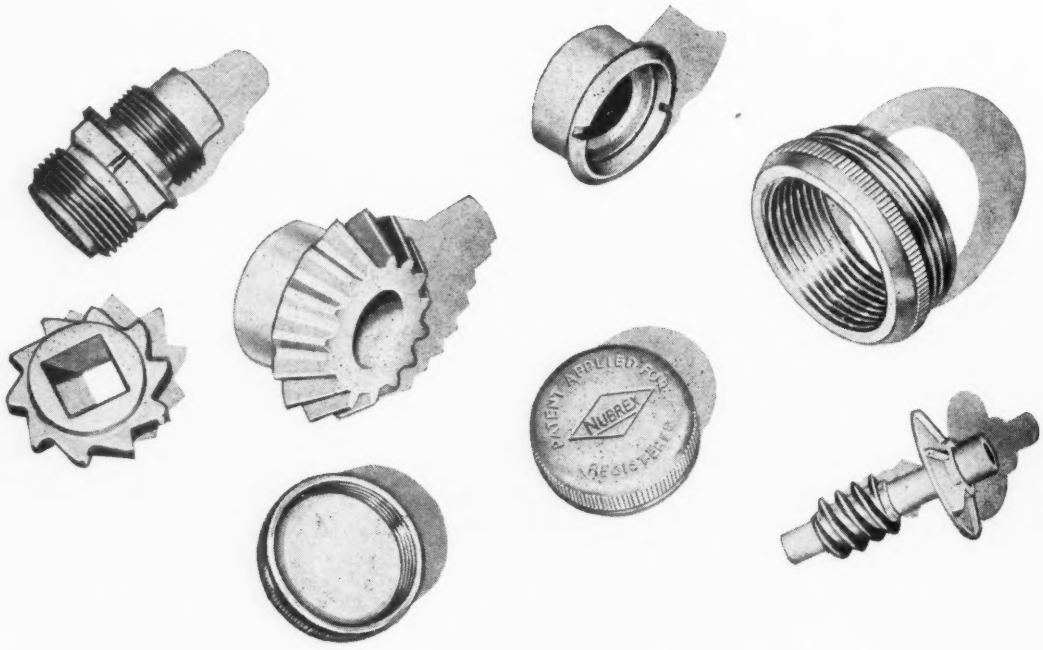
TODAY'S PROBLEMS IN INDUSTRY

Avoiding Waste



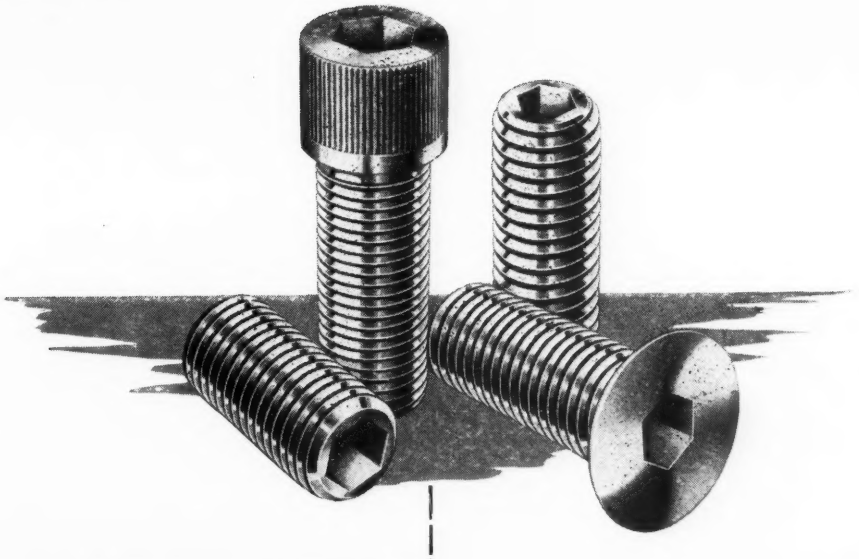
The zinc die castings shown here were cast nearly, if not exactly, to their final form with practically no waste. If they had been machined from bar stock each would have yielded a high proportion of scrap — saleable, yes, but at a considerable loss. Where the quantities are sufficient — and here a die caster's advice must be sought — die casting in zinc alloy can lead to economy in many components like these. As the illustrations show, slots and lettering can be cast in with no need for additional processes. External threads are easily cast but internal threads are often machined where this is quicker than unscrewing cores. In addition strength is good and dimensions are closely held so that only fast, simple finishing operations are needed. The die caster remelts the sprues and runners and thus hardly any surplus metal remains to be disposed of. Cases like these prove that die casting in zinc alloy is the shortest distance between raw material and finished product.

Write for list of members and publications describing the properties and uses of die castings to :—

ZADCA

ZINC ALLOY DIE CASTERS ASSOCIATION

34 Berkeley Square, London, W.1. Telephone: GROsvenor 6636



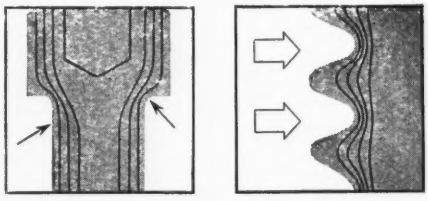
Let's see what UNBRAKO SOCKET SCREWS MEAN TO YOU

On the right we tell you, if you are interested, how we do it. Here we want to say what Unbrako screws mean to you. They mean safety. We all know the old rhyme about how the battle was lost for want of a horse shoe nail. In these mechanised days it is very much easier to lose a reputation because of the consequences of bolt failure. Unbrako screws mean new, lightweight designs that you dreamed about, because fewer, smaller Unbrako screws will take the strain, enabling you to make lighter, smaller joints. Unbrako screws mean economy, with fewer screws to the job, less drilling, less tapping, less labour. Unbrako screws mean better design, because no spanner clearance is necessary and screw spacing is determined by engineering considerations only. Unbrako screws mean greater efficiency, bigger business, more prosperity to you. And one of our technicians will be very happy to call, without obligation, to go into more detail.

UNBRAKO

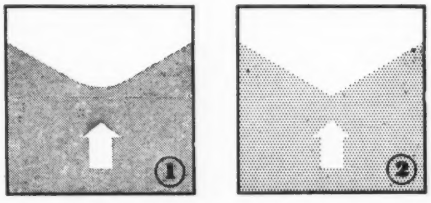
UNBRAKO SCREWS COST LESS THAN TROUBLE

Unbrako screws are made from their own special formula steels and carefully controlled through every stage of manufacture to produce the world's finest fastener. Better steel means that Unbrako can give you a deeper socket for greater wrench purchase, and greater wrench purchase means tighter screws, a safeguard against stress and vibration.

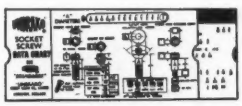


Most screw failures occur at the junction of the head and shank or at the thread root. Unbrako design and manufacture ensures maximum fatigue resistance at these vital points, giving you stronger, safer screws that will never let you down.

Unbrako develop fully formed threads with a radius instead of crack-inducing corners. The metal is compressed to a closely knit grain structure, the grain flow following the contours of the threads, reducing screw failure to a new minimum only possible with Unbrako—the world's finest screw.



Another Unbrako safety refinement. The cone point on Unbrako Socket Screws has a radius, giving firmer seating (1) and precluding any possibility of the weakness to be found in pointed cones (2).



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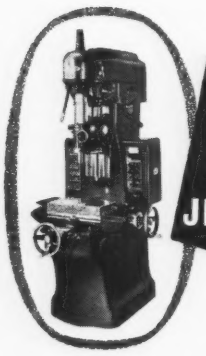

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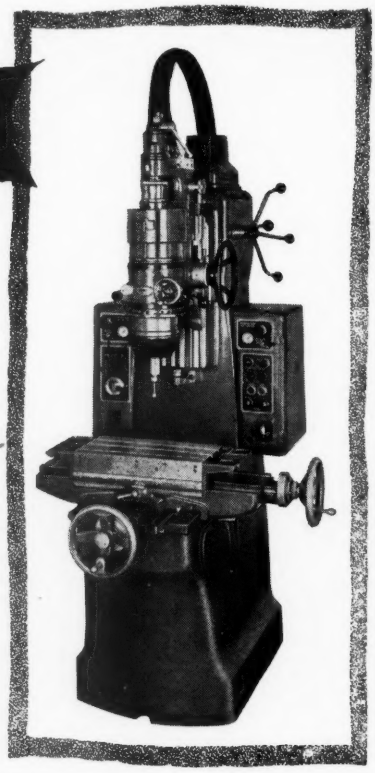

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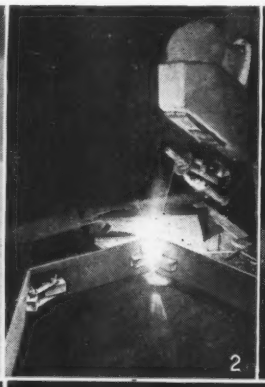

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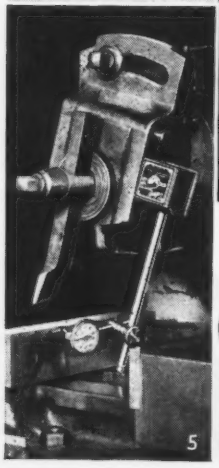

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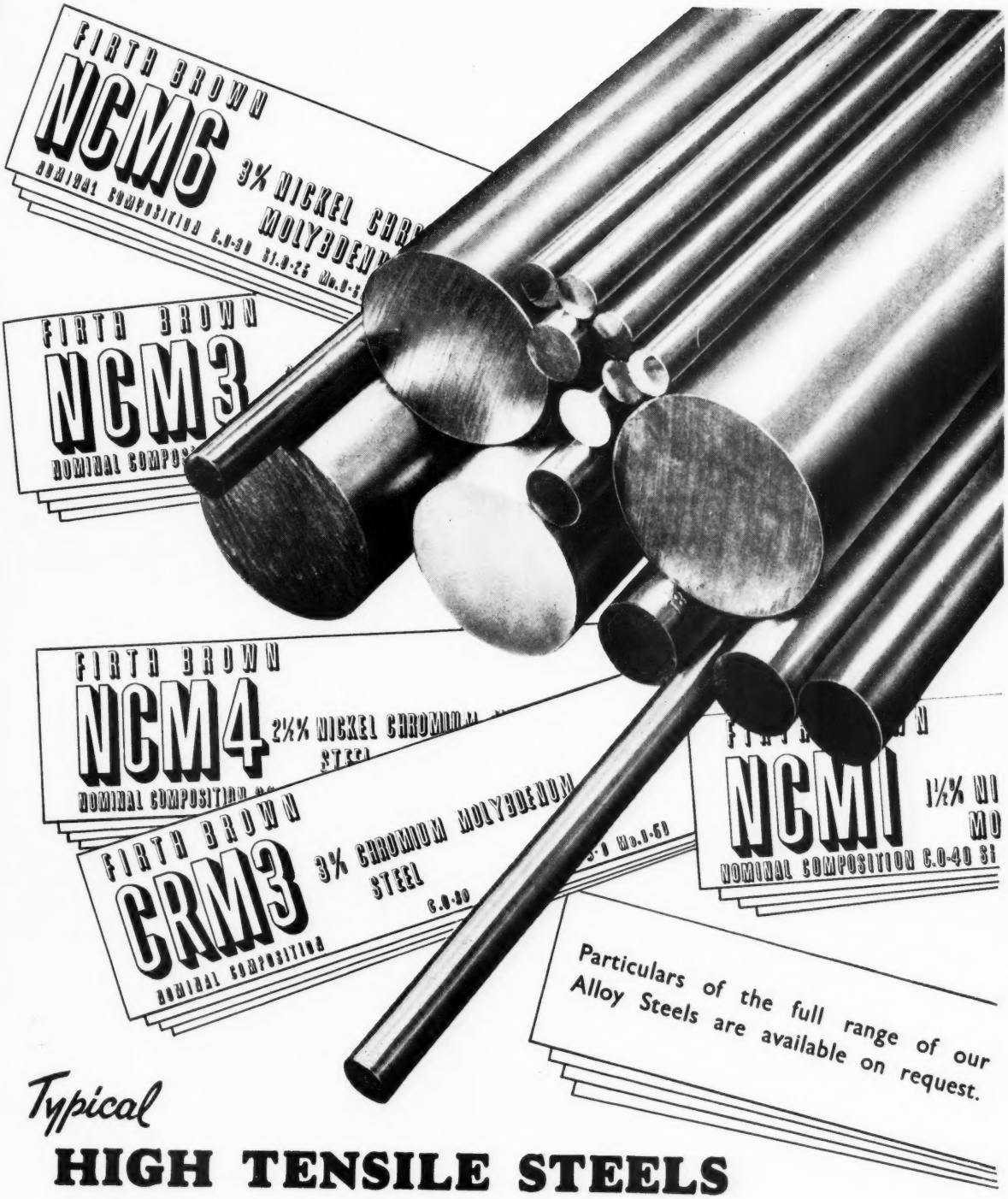

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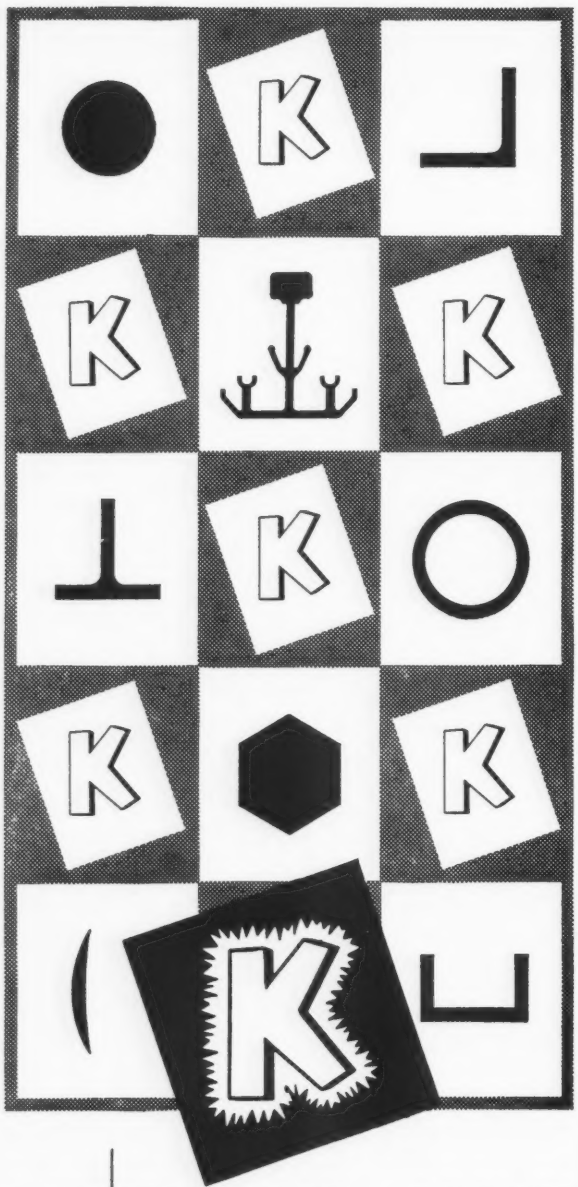

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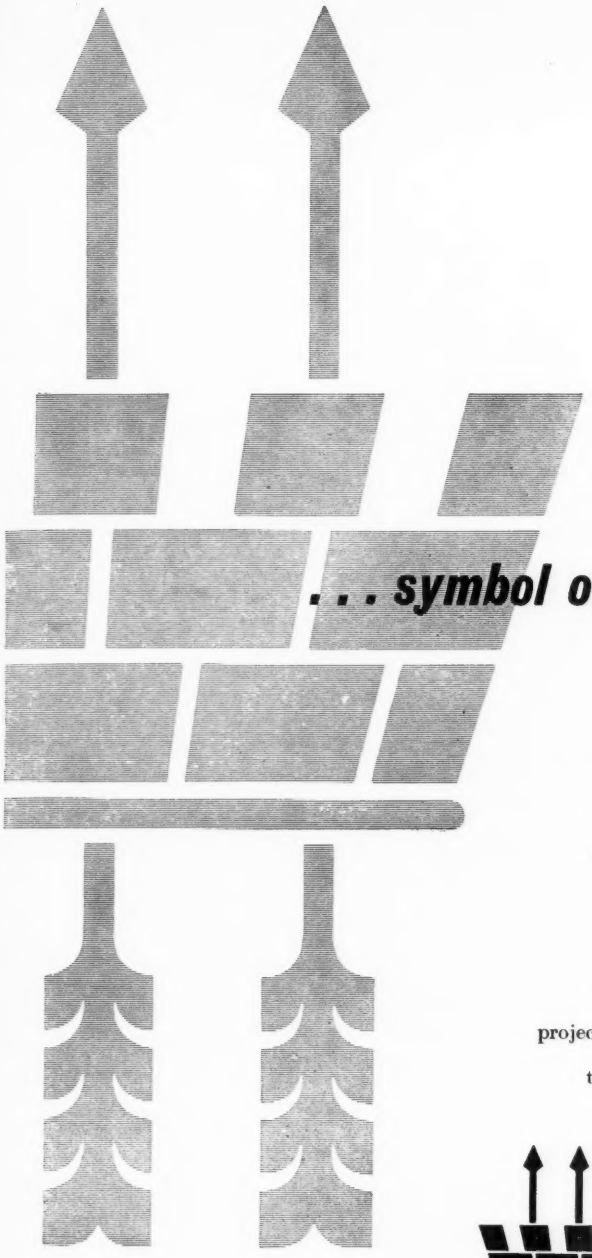

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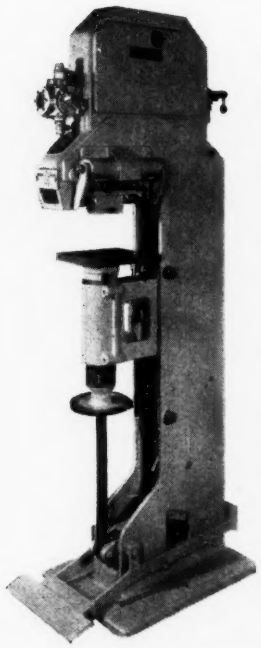
may we present

OUR LATEST E.P. 34 A AIR-OPERATED MARKING MACHINE



This machine, the latest addition to the PRYOR range of marking equipment, simplifies and speeds up the marking of an extremely wide range of cylindrical and flat components. Air operation ensures exact uniformity of impression and reduces operator fatigue to a minimum. A full range of type holders, and geared roller-die carriers is available. Full details of this machine and of our hand-operated models will be sent on request.

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- Can be supplied for "GO" only, or "NOT GO" only, or both "GO" & "NOT GO" combined.

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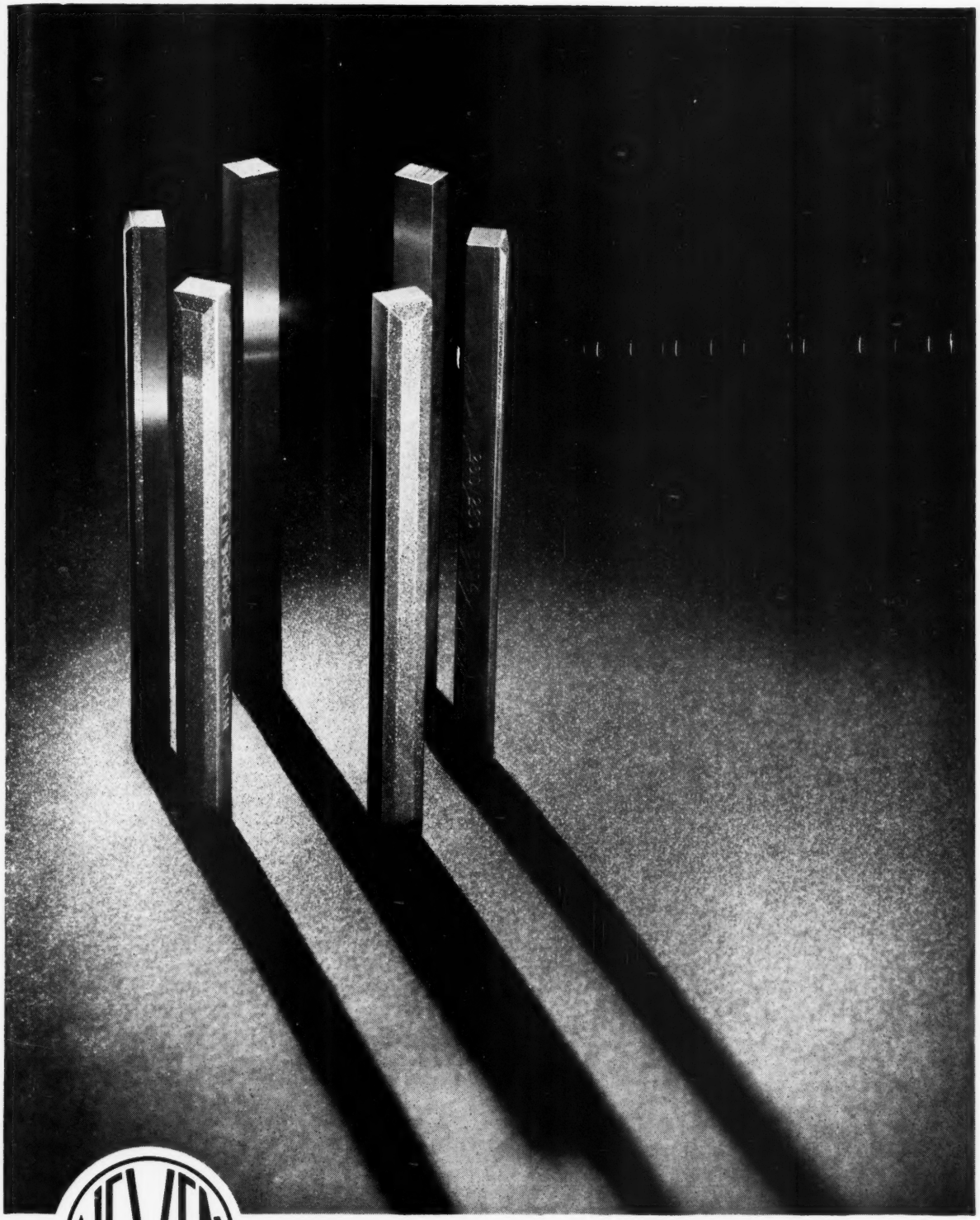

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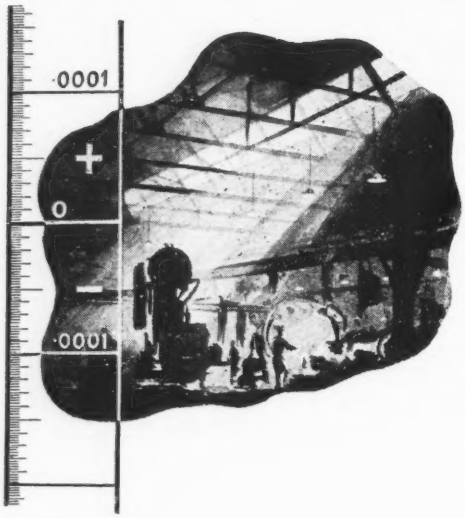

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A new HIGH SPEED milling technique for

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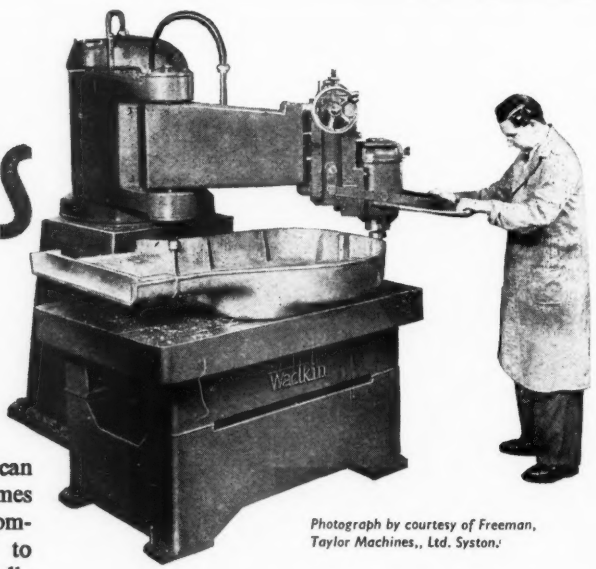
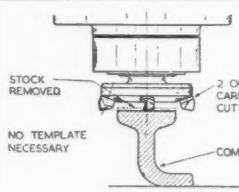


Photograph by courtesy of Freeman, Taylor Machines,, Ltd. Syston.



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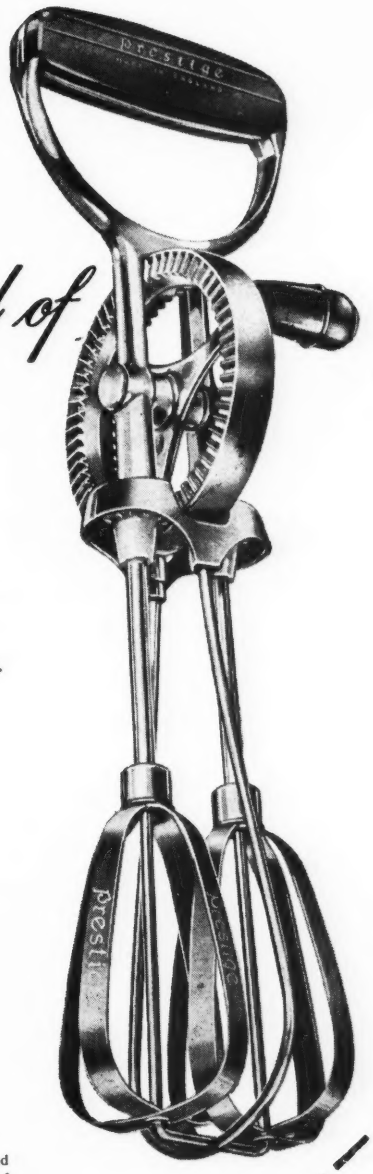

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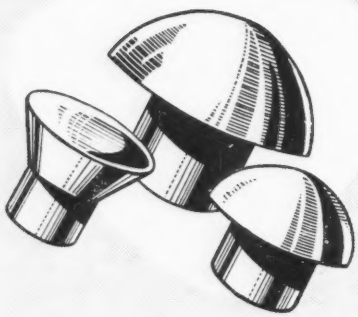

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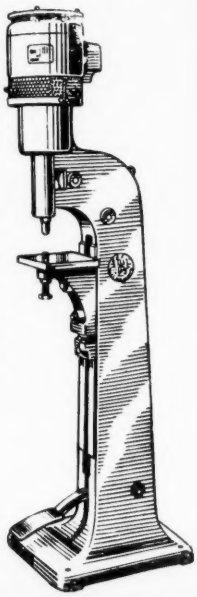

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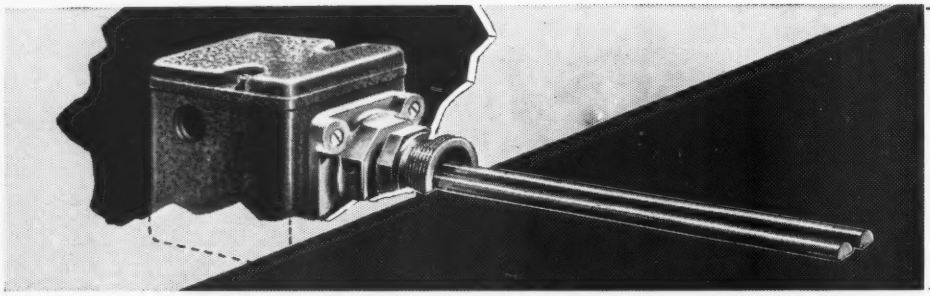

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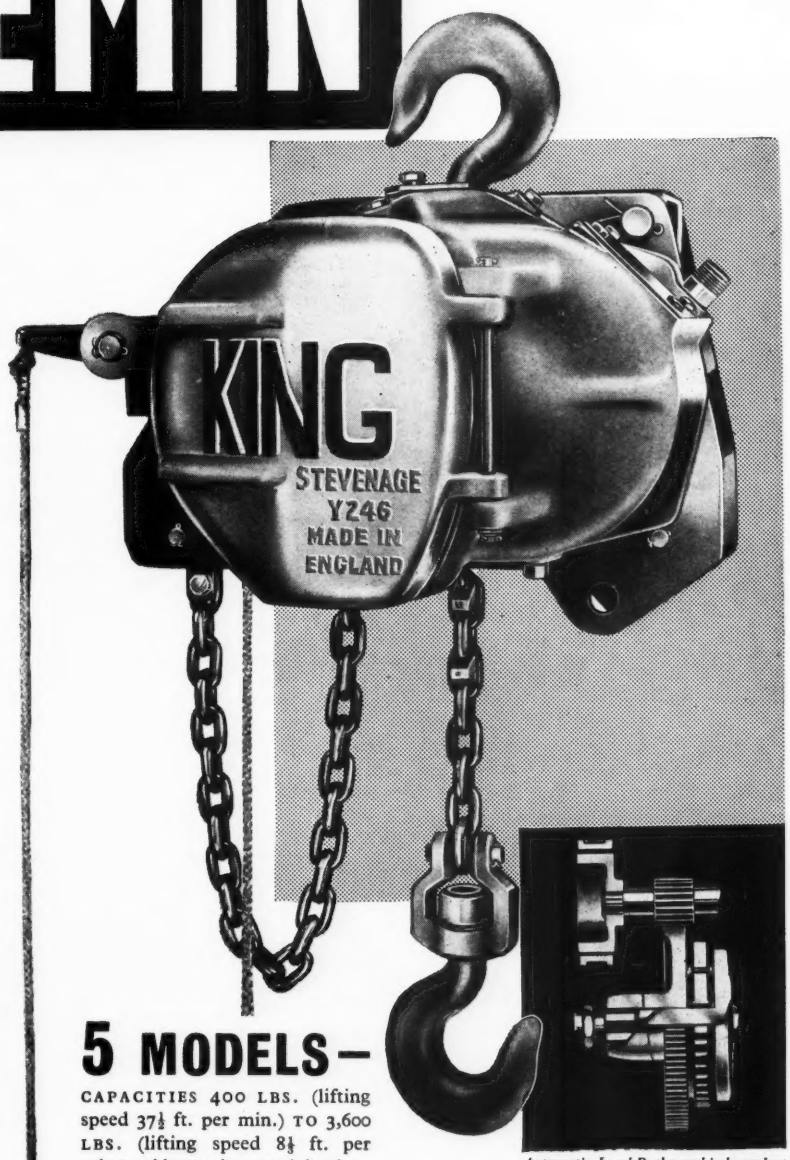

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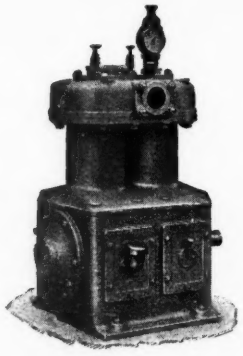

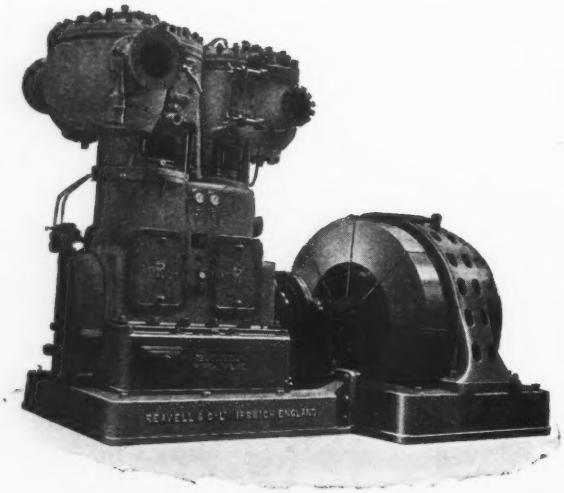

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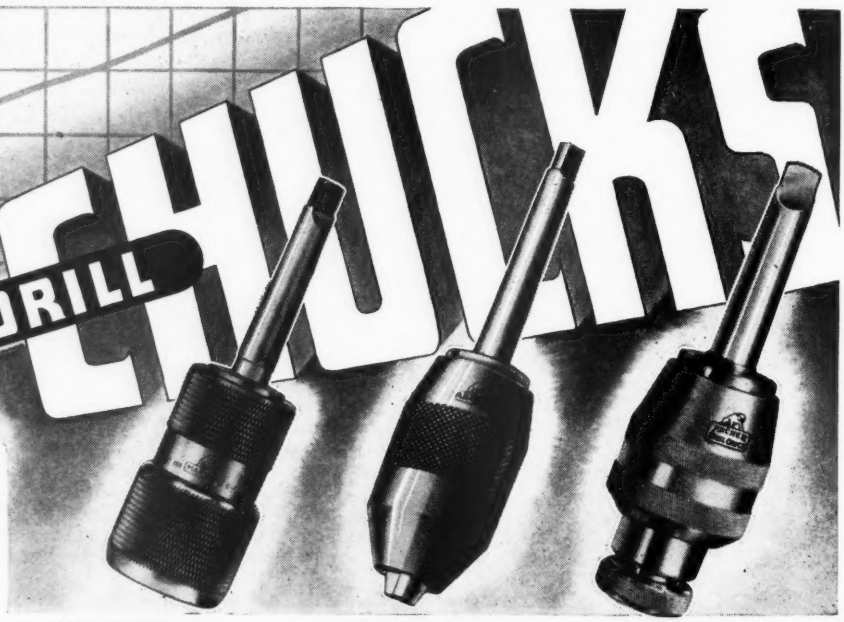

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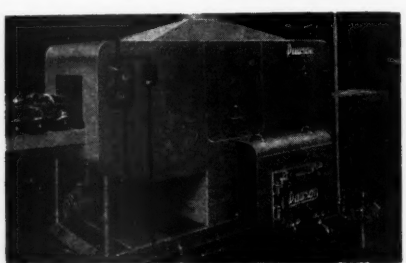
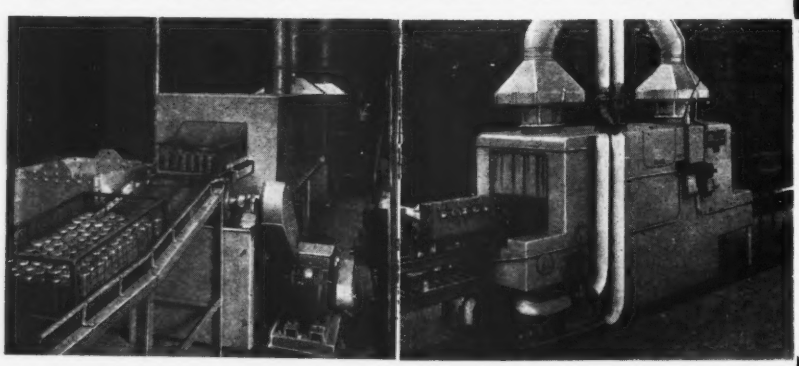
The most practical plant for Cleaning METAL PARTS

PRIOR TO ASSEMBLY, and BEFORE and AFTER REPAIRS



A 'Junior' type machine supplied to a Midlands Motor Car Works.



Supplied for the washing and drying of Tractor parts. Crank Shafts, Sumps, Pressings, etc.

A model 'A' machine, washing parts of motor car engines prior to assembly.

Dawson Metal Parts cleaning machines are supplied for all branches of the engineering industry. Their chief characteristics are robustness of design, small number of working parts and simplicity of operation. Space only permits the illustration of four of the many types of machines built for quick economical washing and drying of Metal Parts.

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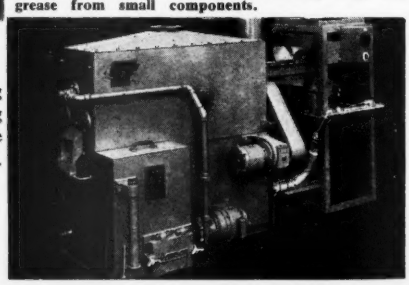
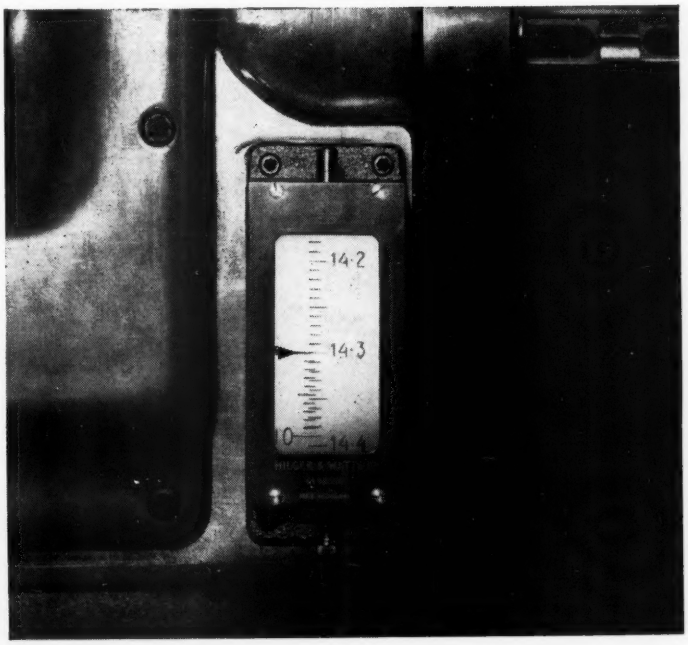
A Rotary Drum machine for removing swarf and grease from small components.



*OPTIMETRIC

TRB

TOOL ROOM BORER



Co-ordinate setting to direct readings of .001″ to ± 0.00025″ (or .01 mm within .005 mm) without eyestrain or fatigue to the operator is a feature of the Kearns Horizontal Tool Room Boring Machine No. 0 Series—described and illustrated in catalogue TRB 3

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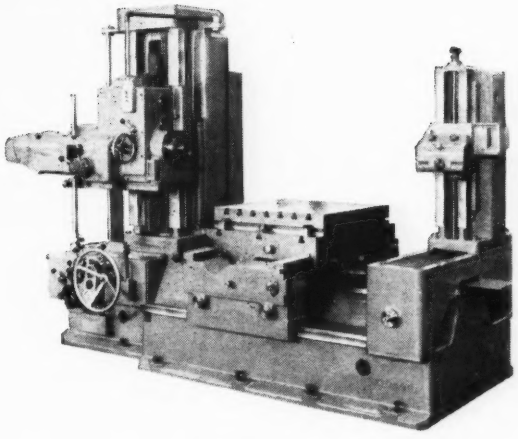
is the registered trade mark of Kearns system of optical measurement fitted as standard to all Kearns Horizontal Tool Room Boring Machines



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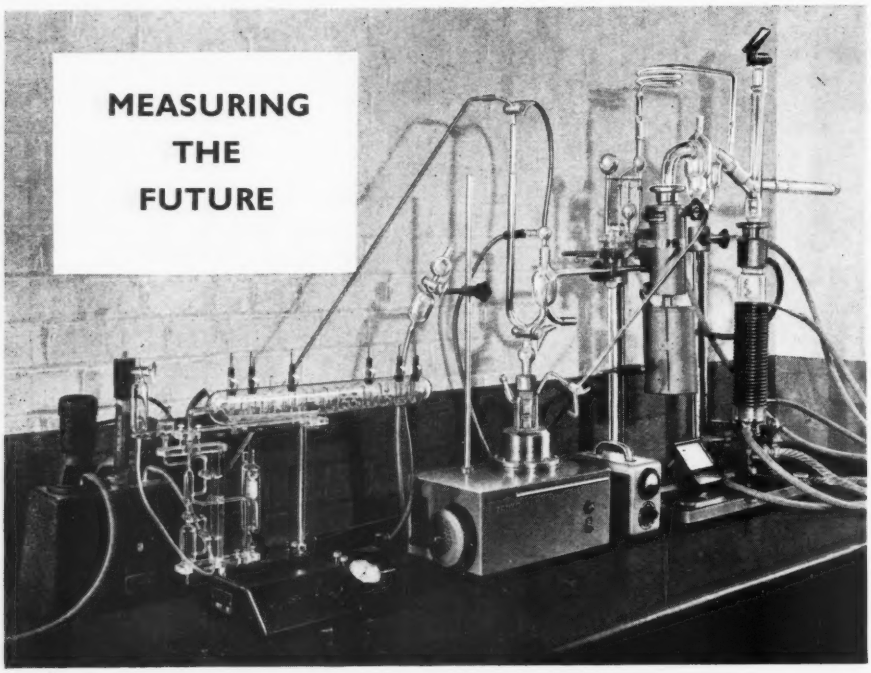

The adoption by this Company of advanced techniques for processing hard metal cutting tools in high vacuum has led to intensified research in this field. We show here a hot-extraction micro gas analyser which measures and analyses the minute amounts of gas found in cutting tool materials. The measurement and control of this factor is leading to better hard metal.

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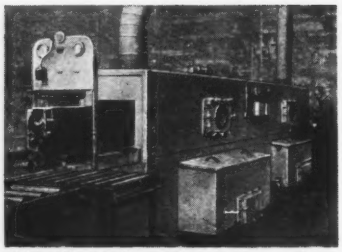
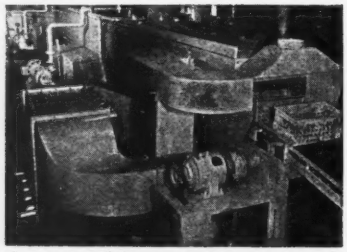
INDUSTRIAL CLEANING MACHINES

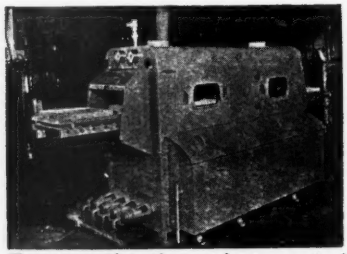
can be designed to meet your particular cleaning problems



This illustration shows a machine cleaning crank cases in the production line. It is equally capable of cleaning small parts in baskets.



A power driven conveyor system is employed with this cleaning machine for ball bearings.



Trays carrying the work are pushed through on a roller conveyor by hand in this cleaning installation.

Whilst offering a very wide variety of standard cleaning equipment, it is BRATBY policy, wherever possible to design the machine to meet the particular cleaning problem. Careful study of each individual problem ensures maximum efficiency and economy of the plant in operation. The illustrations show but a few of the specific types of Cleaning Machines designed by BRATBY for individual needs.

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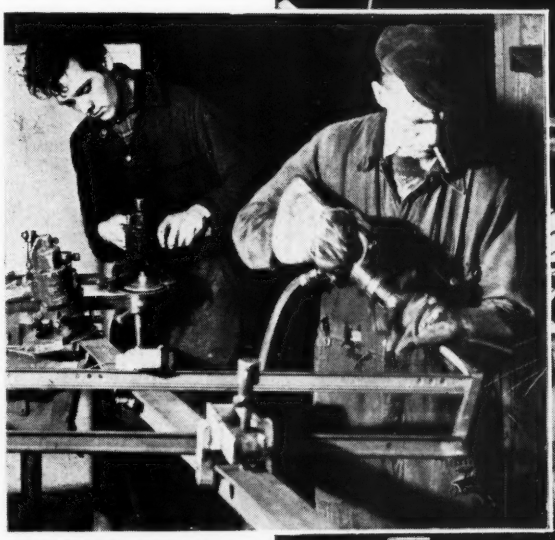
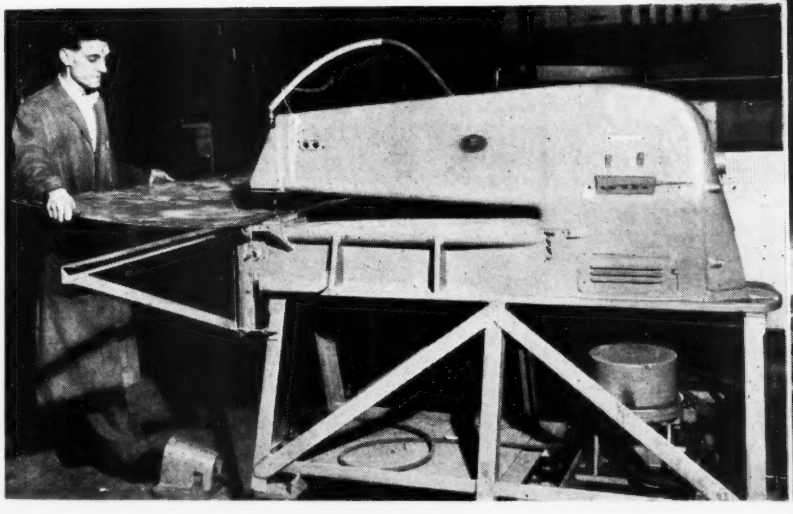


"BROOMWADE" DXIC Chipping Hammer dressing welds on window frames and "BROOMWADE" PR2 Sander fitted with unilastic wheel. Courtesy: Luxfer Ltd.

"BROOMWADE" HPS5 Squeeze Riveter counter punching a Bus side stress panel. This tool exerts a 5 ton load at the snaps. Courtesy: Park Royal Vehicles Ltd.



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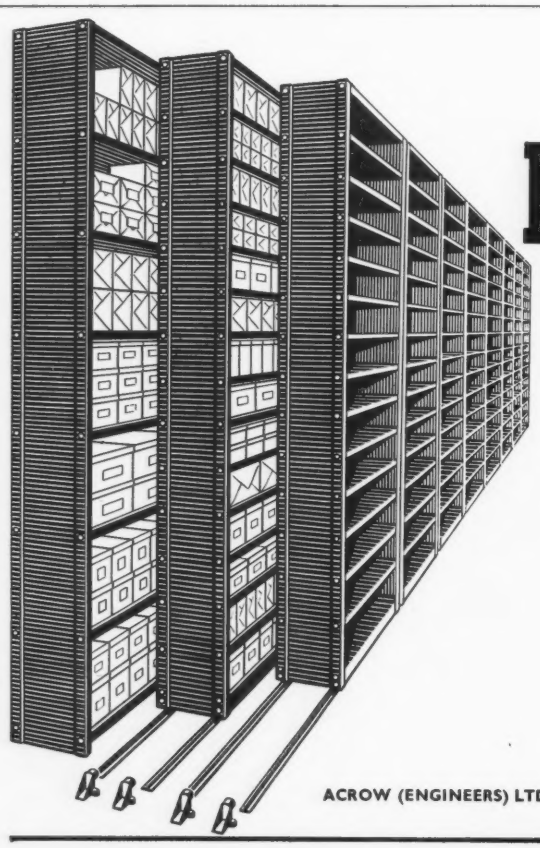

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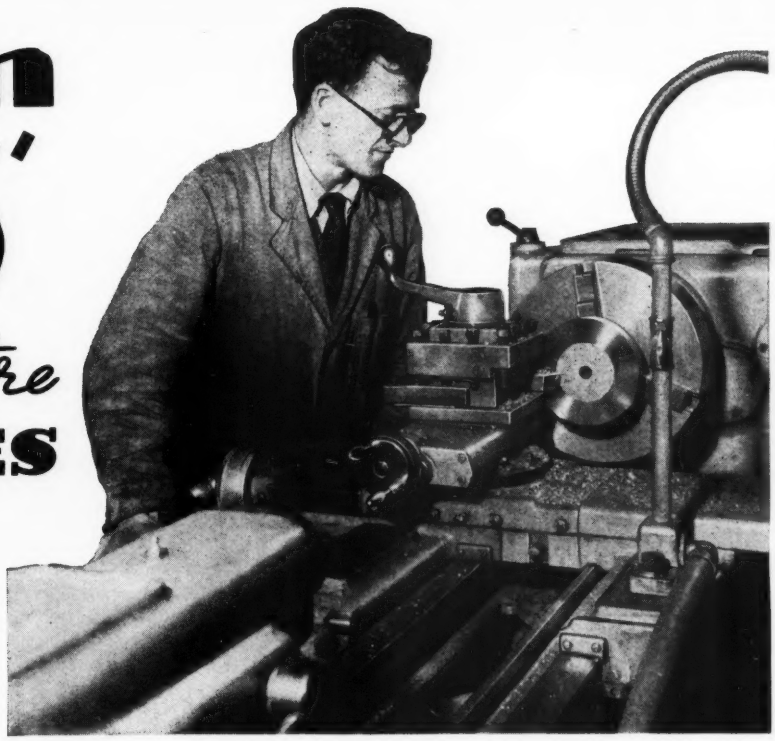

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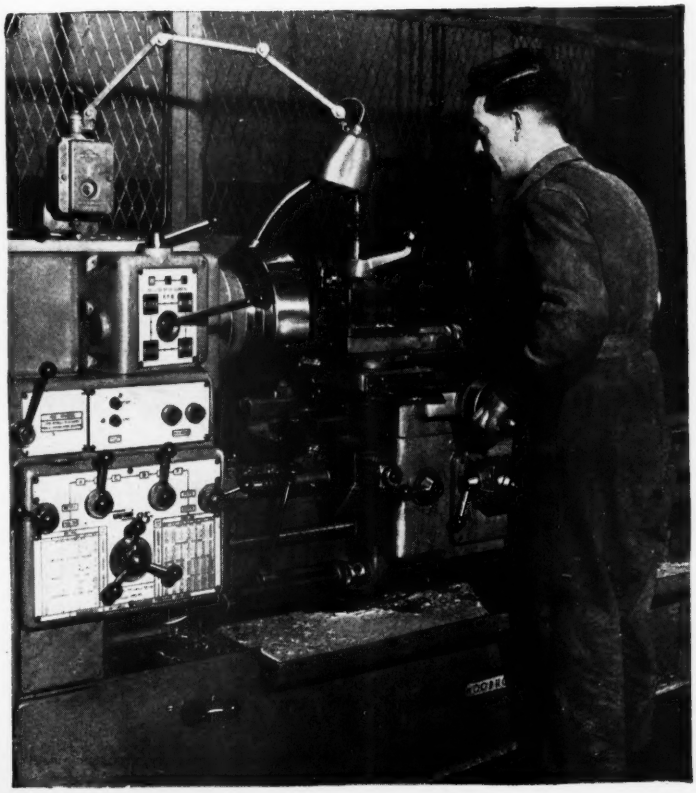

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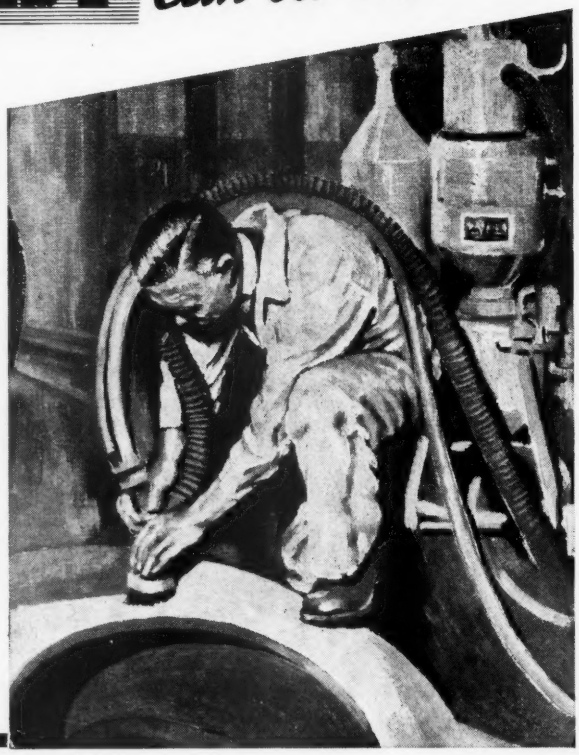

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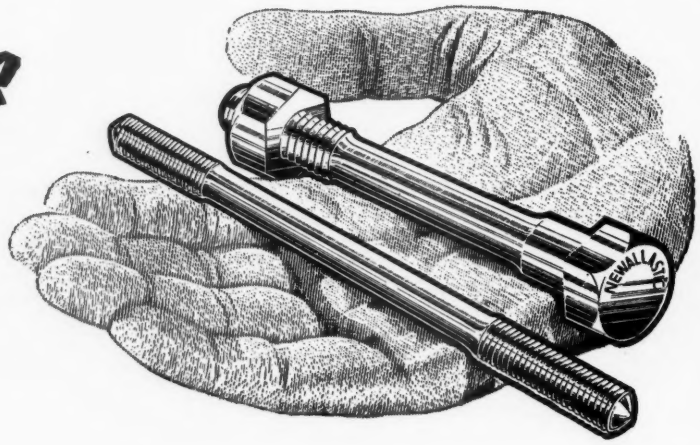

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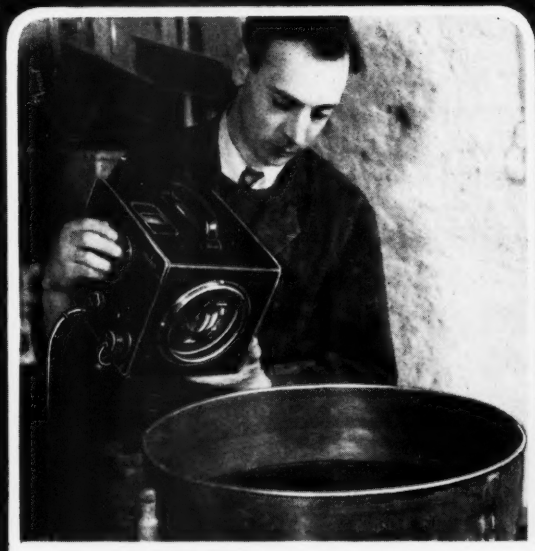

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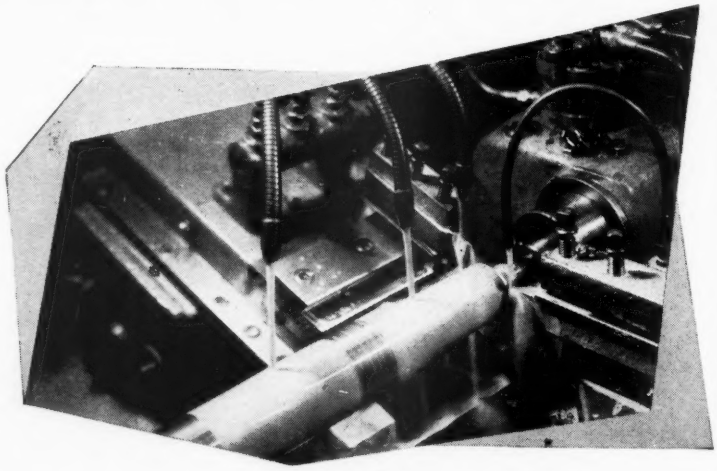

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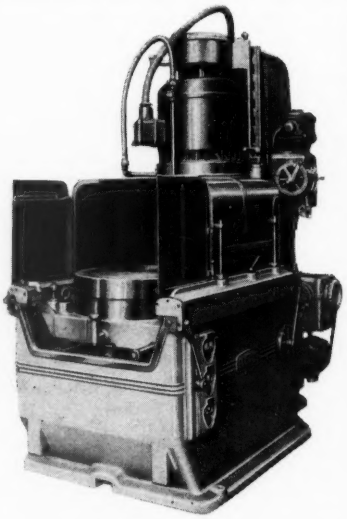

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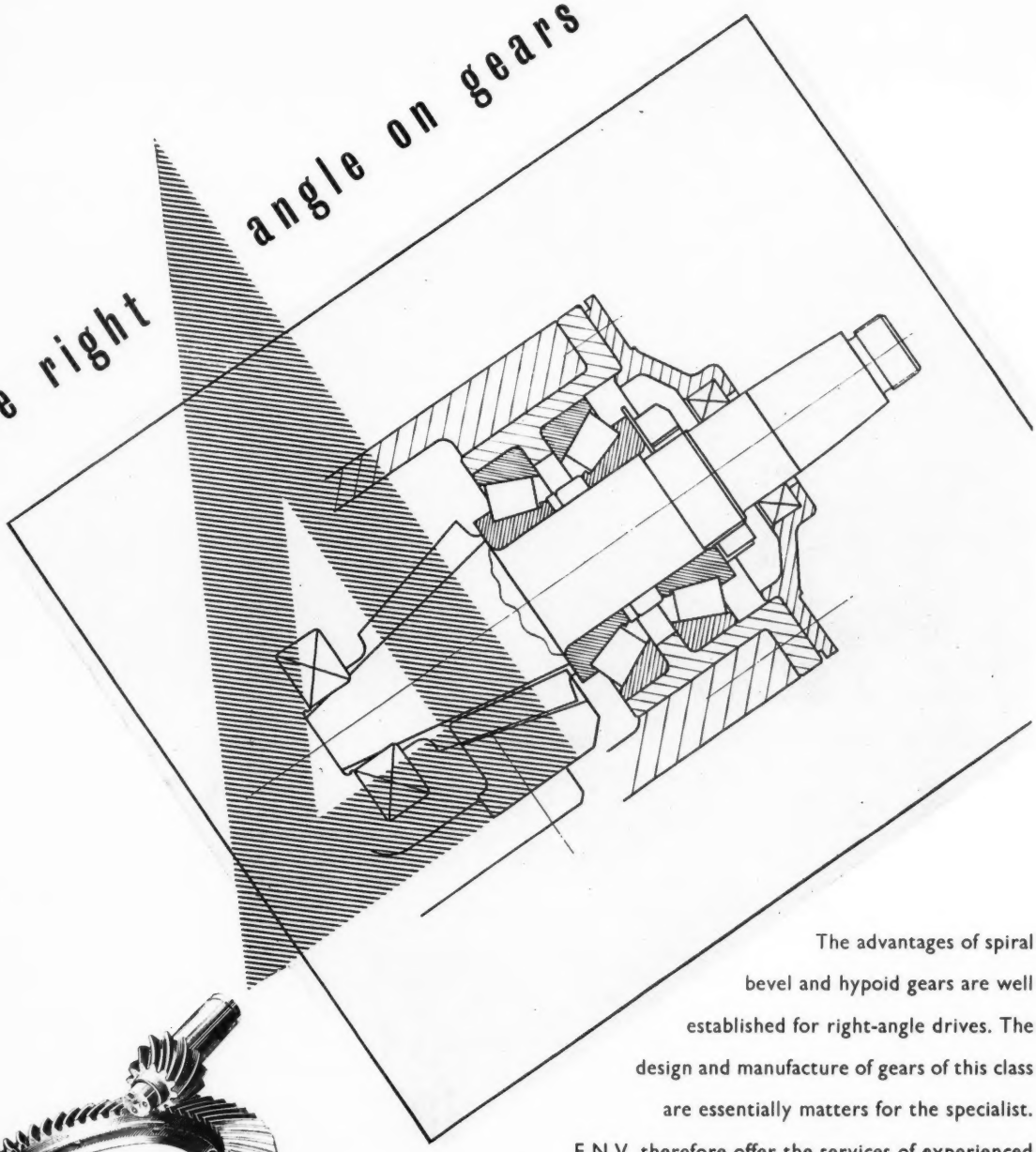

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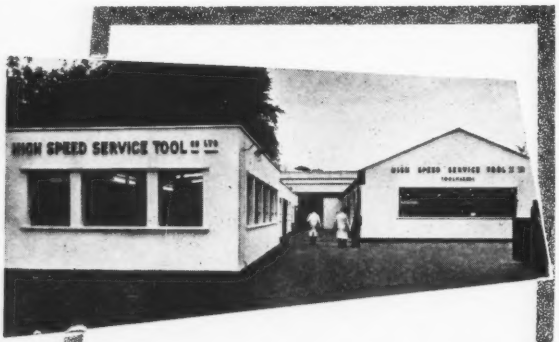

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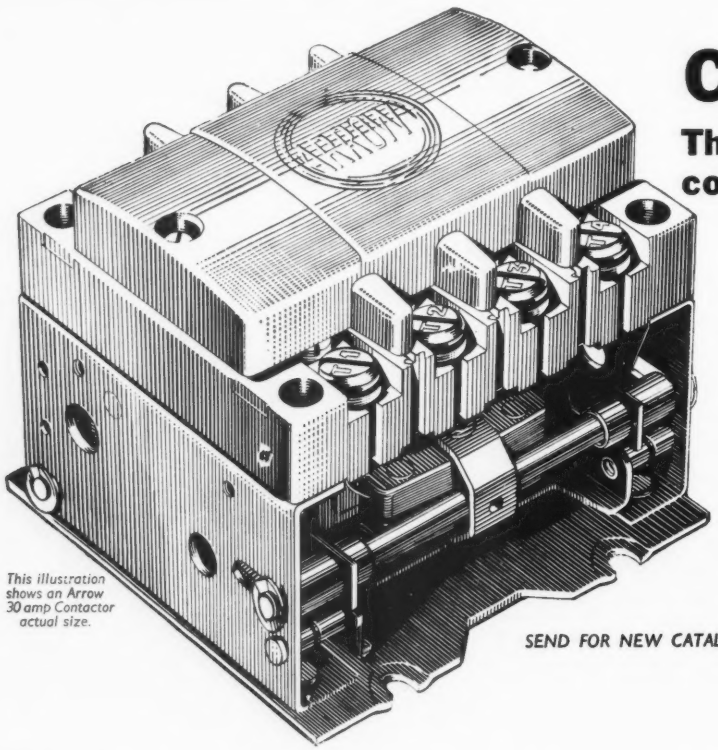

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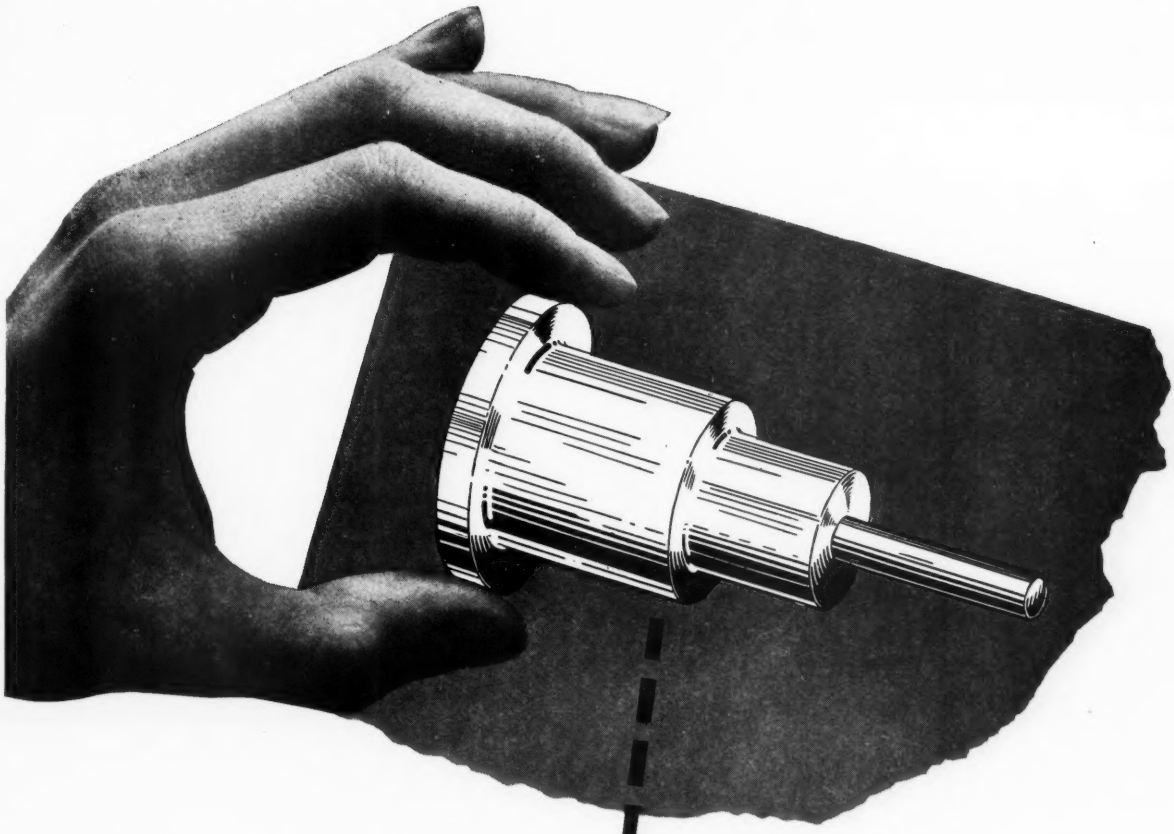

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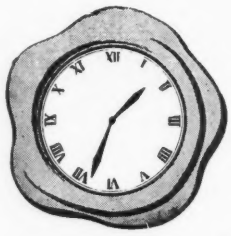

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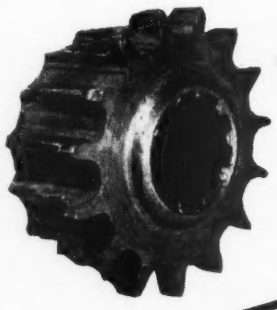
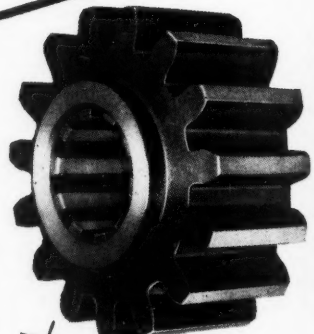


...this badly worn spur gear



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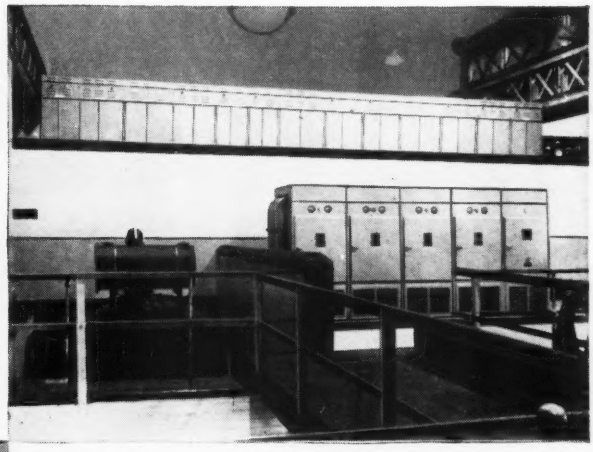
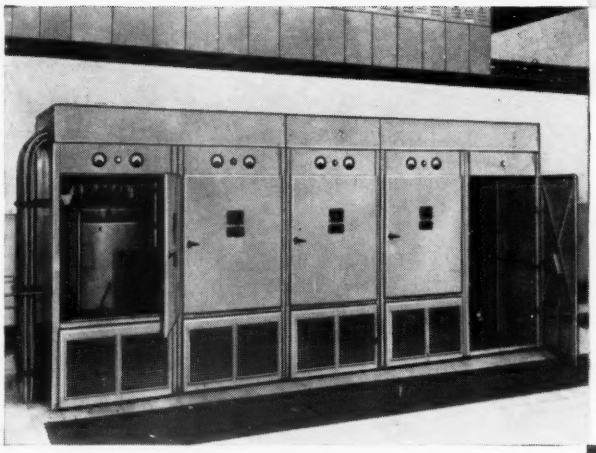
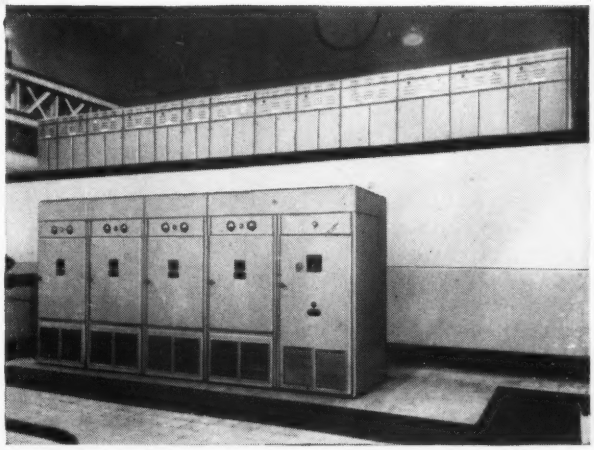
METROPOLITAN VICKERS Rectifiers at Work





in steelworks



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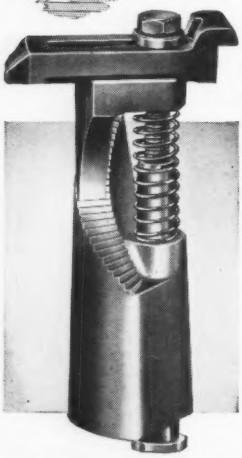

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1/2	3/8 - 1 3/8"
1/3	1 1/2 - 3"
1/4	2 3/8 - 5 1/4"
1/5	4 3/4 - 7 3/8"
1/6	7 - 10"

SIZE 2 for 9/16", 5/8", 14 and 16 mm. 'T' slots

No.	Heights
2/1	0 - 1 3/8"
2/2	3/8 - 1 3/8"
2/3	1 1/2 - 3"
2/4	2 3/8 - 5 1/4"
2/5	4 3/4 - 7 3/8"
2/6	7 - 10"

SIZE 3 for 11/16", 3/4", 17 and 18 mm. 'T' slots

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3/2	1 - 2 3/8"
3/3	2 - 4 3/8"
3/4	4 - 8 3/8"
3/5	8 - 12 1/2"

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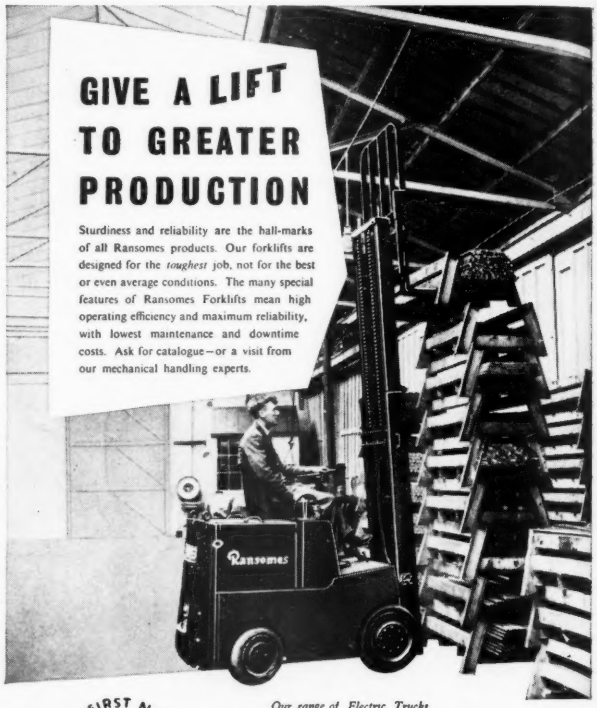
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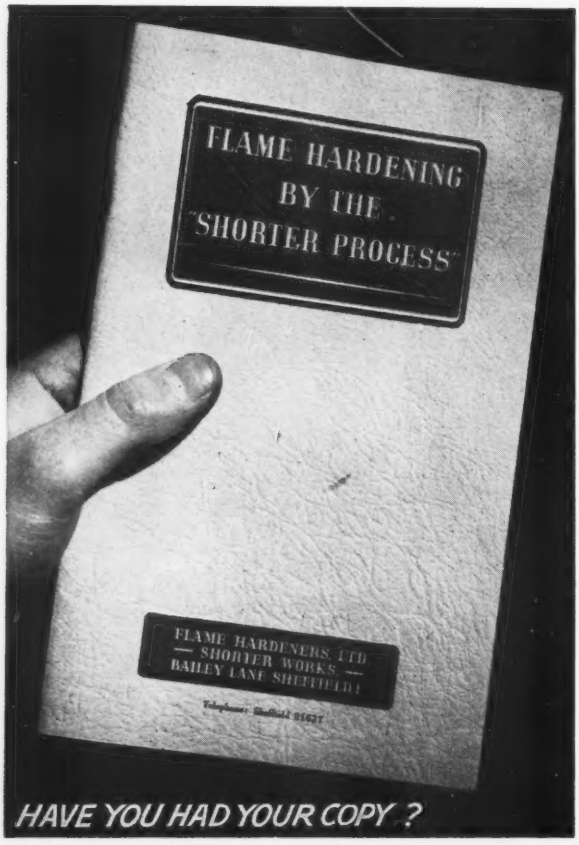
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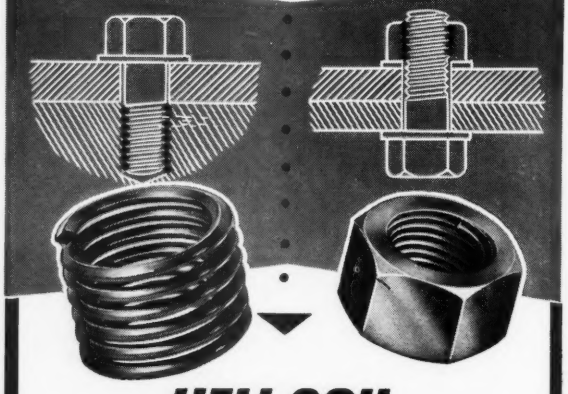

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1st December, 1954.

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Dear Sirs,

As promised we give below the comparable life of the Chilled Iron, Tungsten Carbide and Carbon Tetra Boride Nozzles when using Aluminous Oxide Grit:

Chilled Iron	1 - 2 hours
Tungsten Carbide	8 - 10 days
Carbon Tetra Boride	3 - 4 months

We are also forwarding to you under separate cover one used Carbon Tetra Boride nozzle for your examination; this nozzle having given us four months useage.

We might also add that you are free to quote in your publicity campaign as having obtained these figures and thank you for your co-operation.

Yours faithfully,
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CTB nozzles outlast them all

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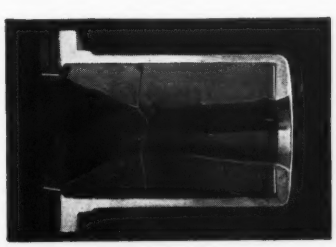

A $\frac{5}{16}$" bore Carbon Tetra Boride Nozzle with tapered lead-in after four months use with alumina shot.
Note:—The bore at the nozzle throat is only $\frac{1}{32}$" larger than when new.

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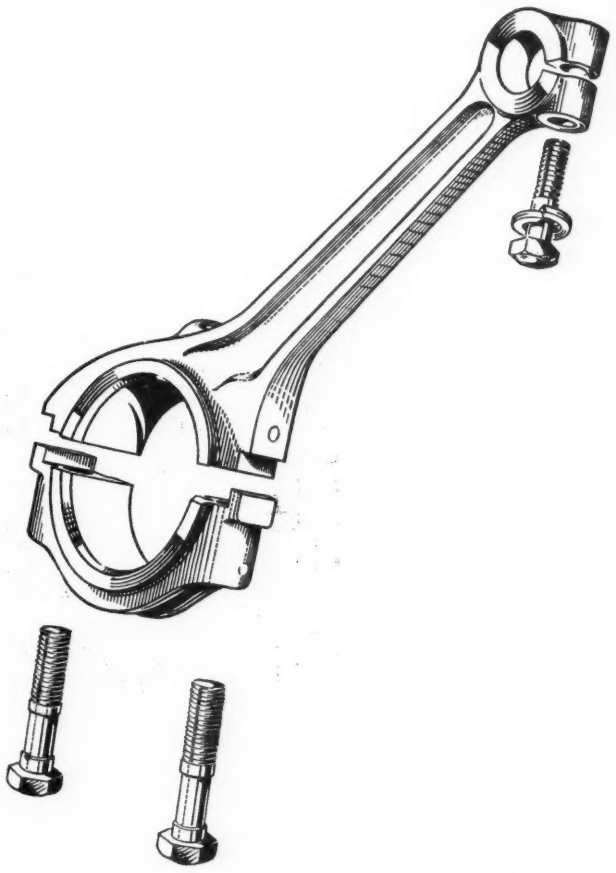

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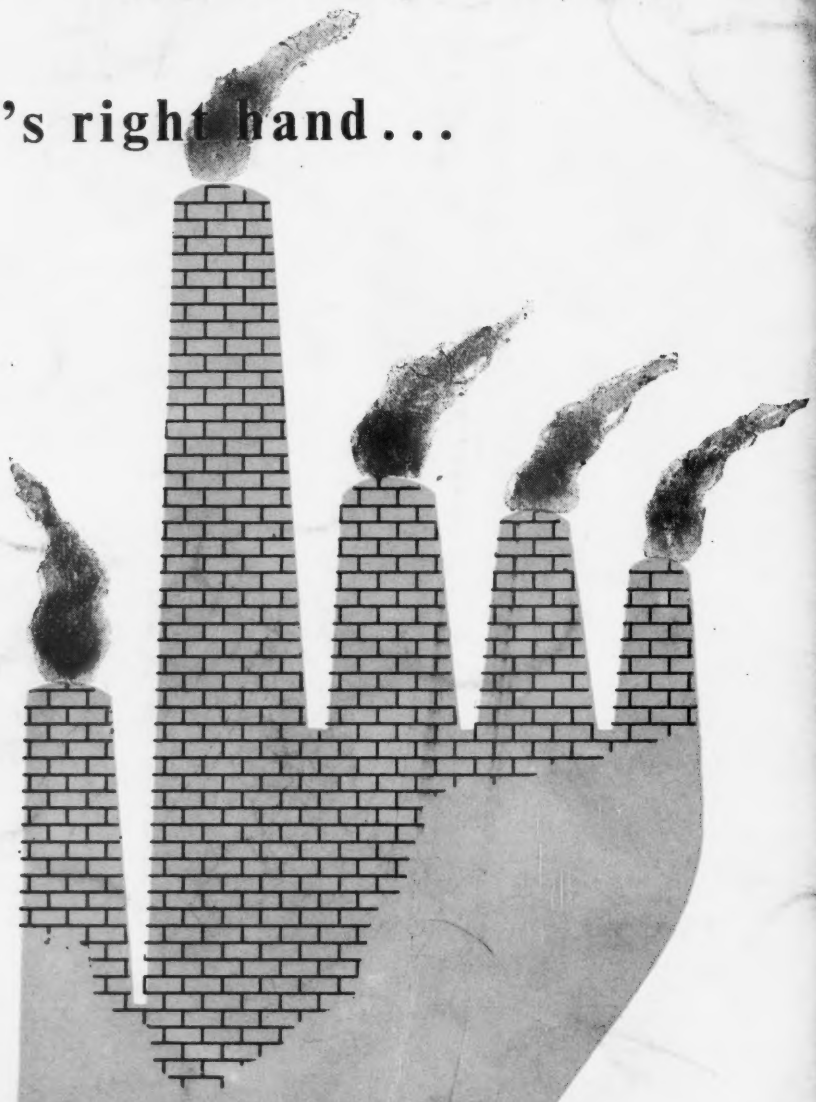

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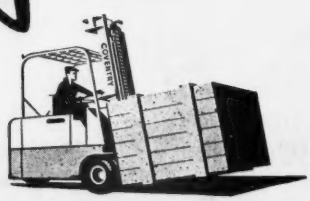

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